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PART I.

LATE on the night of the 23rd of April there died at Leipzig the great physiologist and teacher, Carl Ludwig, one of the most distinguished savants of the present century. His name will ever remain a distinguished one on the bede-role of science, while the goodness, kindness and the singular amiability of his character will be cherished in the hearts of his numerous pupils, friends and admirers. Although the writer was personally very intimate with him, he feels strongly the difficulty of presenting a picture of the life, personality and work of Ludwig.

Carl Friedrich Wilhelm Ludwig, to give him his complete name, although he always signed simply C. Ludwig, was born on 29th December, 1816, on the banks of the Weser, in Witzenhausen, near Cassel, in Kurhessen. He attended the gymnasium in Hanau and subsequently studied medicine in Marburg and Erlangen, and became doctor of Medicine in Marburg in 1839 at the age of twenty-three. To the end of his days he bore the mark of a cut on his upper lip, a memento of his vivacious youth. In Marburg he became successively Prosector of Anatomy (1841), and Privat-docent of Physiology (1842), and Professor (Extraordinarius) of Comparative Anatomy in 1846. In 1849, he was called as Professor of Anatomy and Physiology to Zurich. From Vienna came an in-

vitation in 1855 to occupy the Chair of Zoology and Physiology in the so-called "Josephinum," which was a school for military surgeons, but which was afterwards abolished. There he remained for ten years, until towards the end of 1864 he was called to Leipzig, to which city he migrated in 1865 to become the successor of Ernst Heinrich Weber. At Leipzig Ludwig remained until his death. Here also for the first time he had an opportunity of building a new Institute of Physiology after his own plan, and here, during three decades, his extraordinary activity and his genius manifested themselves, so that soon his fame as an original investigator and teacher became known throughout the scientific world. The "Physiologische Anstalt" in the then Waisenhaus Strasse (now Liebig Strasse) became and remained for three decades perhaps the greatest centre of physiological activity in the world. Thither there flocked from all lands medical men who had already completed their curriculum, to study and prosecute research under this distinguished master. The "Anstalt" or "Institut" was constructed in the form of a large Roman E. From the centre of the vertical bar there projected the Lecture Theatre, so that all the rooms were thus well lighted. It consisted of three storeys. In the uppermost lived the professor and his family, and his faithful friend and mechanic Salvenmoser; in the lowest were the rooms for the laboratory servant, and numerous apartments for the gas-motor and other apparatus, and for animals, while the middle storey was the true workshop, and contained rooms for histological, chemical and experimental physiology.

When one talks of Ludwig's "pupils" one means more than the ordinary student of medicine who attended his lectures. What one means is this, *viz.*, that Ludwig not only taught medical students, but his pupils, or rather "young friends" as he called them, were medical men who, attracted by his fame as a teacher, came to his laboratory to pursue under his direction original research. In the laboratory during the session there might be ten to fourteen pupils, each pursuing an independent theme under his guidance.

Nearly all the younger generation of physiologists in Great Britain, America, Italy, Germany and Russia have at one time or another studied in his laboratory. The results were published annually in the famous and classical *Arbeiten aus der physiologischen Anstalt zu Leipzig* during the years 1866-77, *i.e.*, for eleven years (Jahrgang 1-11). At the foundation of Du Bois-Reymond's *Archiv* in 1877, Ludwig agreed to publish his work and that of his pupils in this journal, and so the independent issue of the *Arbeiten* as such came to an end. The results, however, were communicated to the *Sächische Akademie*, which in many cases contributed funds for the illustration of some of the monographs. Nearly 300 pupils studied under him, while the twenty volumes of *Arbeiten* represent much of the progress of physiology during the last three decades. To give an account of the work done by Ludwig and his pupils would in fact be to write the advances made in physiology during the last half-century.

To enumerate his continental pupils would be to name most of the professors of this subject, *e.g.*, Fick (Würzburg), C. Eckhard (Giessen), Kronecker (Berne), Drechsel (Berne), Gaule (Zurich), Lothar Meyer (Tübingen), Miescher (died August, 1895) of Basel, von Kries (Freiburg). In Russia, Setschenow, Woroschiloff, Tscheriew. In Sweden, Holmgren (Upsala), and Tigerstedt (Stockholm), Loven. In Belgium, Heger (Brussels), Lahousse (Ghent). In Italy, Mosso (Turin), Luciani (Rome), Fano (Florence), Baldi, Novi, and others.

Amongst his English and American pupils may be mentioned Lauder Brunton, Coats, Bowditch, Rutherford, Moseley, Ray Lankester, Stirling, Minot, Gaskell, Ward, Haycraft, Cash, Sewall, Meade-Smith, Wooldridge, Walton, Beevor, Buckmaster, Carslaw, F. S. Lee, F. Mall, W. H. Thompson and Vaughan Harley.

Ludwig's family life was one of the happiest that could be wished, and this, perhaps, partly because the circle was so small. He married in 1849, his only son died in infancy, while his only daughter became the wife of Alfred Dove, then the editor of *Im neuen Reich*, and now known as a dis-

tinguished historian and *litterateur*. His intimate personal friendship with many of the leading scientific and literary men of his time was to him a source of great joy. From the old Zurich days dated his intimate friendship with Th. Mommsen, in Leipzig. Apart from his warm friendship with his colleagues, more especially Professors His and Braune, he became the warm friend of G. Freytag, while to his intimate friends and compeers, the three great pupils of Johannes Müller, *viz.*, Helmholtz, Brücke, and Du Bois-Reymond, he dedicated his *Lehrbuch der Physiologie*.

During the long period of scientific activity he was the recipient of numerous honours and titles. He was a member of many learned societies, including the "Akademies" of Berlin, Vienna, Paris, Munich, St. Petersburg, Upsala, Stockholm, Rome, Turin. He was a foreign member of our own Royal Society, from which he received the Copley medal. An honour he valued greatly was that conferred on him by the citizens of Leipzig, in making him an honorary burgess of his adopted city. On the 15th October, 1874, Ludwig celebrated his silver jubilee as professor, on which occasion some of his pupils, as is the custom in Germany, presented him with a number of memoirs, which were published under the title *Beiträge zur Anatomie und Physiologie*, als Festgabe von seinen Schulern gewidmet. He was again the recipient of a similar honour on the occasion of his fiftieth year as doctor. During his last illness, and when I went to Leipzig in the hope of seeing him, von Frey reported that his strength was well maintained. At that time it was proposed to have his portrait painted and presented to him on his eightieth birthday, as a small token of the esteem in which he was held by his pupils, but, alas! it was not to be. He died while the arrangements were being made.

Ludwig devoted himself with the utmost assiduity and with untiring constancy to the work of his laboratory, but from time to time he sought relief from the intense strain of steady application to work by short journeys in Germany, Italy, Belgium, Holland and England. For him the natural beauty of the country had a great fascination, while art in

every form was a source of pleasure. How well he could discourse on the beauties of Italian art, on the gems of Van Eyk and the etchings of Dürer was well known to some of us who had the inestimable pleasure of free intercourse with him. Up to the very end of his life he preserved a wonderful freshness and elasticity. In April, 1894, when I last saw him, he seemed to have changed but little in appearance, whilst the same interest in everything new in the scientific world awakened his eager interest. The only sign of age he then manifested was his fear that he was fast becoming old, and that he no longer possessed in the fullest manner the power of doing his duty as he wished to do it. He continued to lecture without discomfort during the session 1894-95, nay, he even conducted the examinations for the degree at the end of the session, when towards the end of March, 1895, he was seized with an attack of influenza, and just when he seemed to be recovering he was suddenly seized with heart failure, late on the night of the 23rd of April, at 11·30 P.M. As he raised himself to take a drink he fell back in bed; and thus passed away in the fulness of years and full of honour, perhaps—nay, the greatest teacher of physiology of this or any century. Although for seven weeks he was confined to his bed, yet he suffered little pain; and at his grave it was said of him as of Moses: "Seine Augen waren nicht dunkel geworden, und seine Kraft war nicht verfallen," so that mentally he remained "young and fresh" to the last.

His love for morphological studies was very marked. He had always a microscope beside him, and whenever he had a spare hour he turned his attention to histology. During the last ten years many beautifully illustrated memoirs were published by the Saxon *Akademie*, dealing more especially with the distribution of blood-vessels. The last of these, the arrangement of the blood-vessels of the inner ear, more especially of the semicircular canals, was being produced by his pupil Eicher, but he died ere the work was finished, and so Ludwig finished it shortly before his own death. One of the earliest and most important anatomical memoirs done under his direction was

by Leber, *On the Blood-vessels of the Eyeball.* Most students are familiar with reproductions from this work in the text-books. His microscopical preparations were always made with the greatest care, and when a drawing of a preparation had to be made it had to be done exactly, and with an artistic touch. He always insisted on having the best draughtsman available. His fine artistic feeling led him to this, and surely had he lived in the times of Vesalius, he, like Vesalius, would have had Titian and his pupils to design his plates. To Ludwig, as to Spallanzani, nature was as a large picture, the most remarkable, charming, and fascinating which could be presented to human view, and great was his joy when he was able to lift the veil from some part of the picture and disclose to view some of its hidden mysteries. Of his pupils who have become distinguished anatomists or histologists we may mention Schwalbe, Froriep, Braune, Flechsig, Krause, Minot and many others.

Physiology is at once an ancient and a comparatively modern science. Haller, the greatest physiologist of last century—and in whose honour Kronecker has named the magnificent new Physiological Institute at Berne, Hallerianum—called it "animated anatomy". Our definite knowledge depends largely on the progress made during the last fifty years, and to this progress Ludwig either directly or indirectly has been one of the most potent contributors. Indeed there is scarcely a chapter in physiology on which he has not left the impress of his genius, and which he has not enriched with new facts. It was indeed a great good fortune that Ludwig dedicated his life to the advancement of physiology. The circumstances were propitious, but even granting this, the form in which the study of the subject presents itself at present is largely due to his activity, to his discoveries, to his method of experimenting, and to the original manner in which he trained numerous young investigators to follow in his footsteps.

From a literary point of view the contributions made by Ludwig in his own name are not numerous, but such

as he chose to issue over the simple signature of C. Ludwig have become classical. His life-work finds expression in the long series of scientific memoirs, which contain the result of his investigations for the most part undertaken along with his pupils, while the memoir itself usually bore only the pupil's name. He published the second edition of his *Lehrbuch der Physiologie* in 1858-61, dedicating it to his three friends, Du Bois-Reymond, Helmholtz and Brücke. The book opens with the striking words, not so striking now perhaps as they were then, when physiology was largely dominated by the so-called vitalistic doctrines of Johannes Müller: "Die wissenschaftliche Physiologie hat die Aufgabe, die Leistungen des Thierleibes festzustellen und sie aus elementaren Bedingungen desselben mit Nothwendigkeit herzuleiten," and his aim was to show that "alle vom thierischen Körper ausgehenden Leistungen eine Folge der einfachen Anziehungen und Abstossungen sind". In other words, he sought to bring the phenomena manifested by living beings into line with those of physical science, and to apply to their elucidation the same methods as are applicable to the study of physical and chemical phenomena. To study or even to regard biological phenomena from a physical and chemical standpoint was at that time still a new conception, yet this idea, as v. Kries well expresses it, formed the "Grundton" of Ludwig's whole scientific bias. Indeed, his first scientific work on the secretion of urine was carried out on these lines, and to the end he remained true to this principle. But Ludwig was well aware that there are many phenomena manifested by living organisms for which there are no directly analogous phenomena in inorganic nature; but the point is, that his endeavour throughout was by anatomical, physical and chemical investigations to arrive at such a knowledge of the organ to be investigated as to secure a basis on which the interpretation of the phenomena manifested by it might become explicable by facts already known. One of the necessities of his whole conception of any physiological process was to obtain a comprehensive and palpable representation of the relation between an

organ and its function. With his striving after physical exactitude, there was no fear of his taking too narrow a view of the activities of an organ, *i.e.*, as a merely physical apparatus. This one may illustrate by his researches on muscle, which he regarded not merely as a physical apparatus; he took a far wider and more comprehensive view, he regarded it as an organ whose histological structure had to be investigated, and its physical characters studied; but even that was not enough, it had to be studied from the point of view of how it comported itself when its blood-vessels were perfused with blood, how its properties changed as its nutrition altered, how it breathed, and how its activities were affected by nervous and other influences. Thus we must give a wide interpretation to his views of the physical and chemical processes accompanying the manifestation of so-called vital phenomena.

One very characteristic aspect of his work lay in his strong morphological bias. The anatomical aspect of a problem always attracted him greatly, and he always had on hand some anatomical and histological investigation from which he derived new inspiration for future physiological research. The distribution of the blood-vessels in various parts of the body had for him a peculiar fascination, their relation to other tissues was ever present to his mind. As a consequence we have the beautiful researches on the origin of lymphatics; the effects of the respiratory movements on the movements of lymph; his theory of the mechanical results dependent on the distribution of the capillaries in the glomeruli of the kidney, and how this affects the passage of fluids from the renal vessels into the urinary tubules; how the blood-vessels behave in contracted and relaxed muscle. To what important discoveries he was led by a study of the arrangement of the heart muscle fibres we shall see presently.

Dealing first with the researches published over his own name, we have the classical paper "Beiträge zur Lehre von Mechanismus der Harnsecretion," which he presented to the Faculty in Marburg in 1842, when he qualified as "Privat-docent" of physiology. In this con-

tribution, after studying the minute structure of the kidney, more especially the distribution of its blood-vessels, and after numerous experiments, he attempted to explain the process of urinary secretion on a physical basis. With his pupil Goll, still later, he showed the dependence of urinary secretion on blood pressure, and hence arose the so-called "filtration hypothesis". In 1844 in *Wagner's Handwörterbuch*, vol. ii., p. 628, his researches, extending to twelve pages, are given under the title "Nieren und Harnbreitung". Just as it was his earliest work, so a study of urinary secretion cropped up every now and again in his laboratory. Ustimowitsch in 1870 showed how curare diminished urinary secretion, how the secretion was influenced by section of the renal nerves, and above all, from the more modern standpoint regarding the secretion of urine, this other important fact that urea when injected into the blood-vessels—the blood pressure being very low, so low, indeed, that no secretion of urine took place —again caused a secretion of urine. This fact, recorded by Ludwig's pupil, is to-day utilised to explain secretion by the presence of "harnfähigen" substances in the blood. In 1893 Grijus showed that "the kidneys produce heat in proportion to the water they excrete"; and again we have the important fact brought out in this paper from a comparative study of muscle, salivary gland, and kidney, that while muscle and salivary glands derive their impulse to the production of heat from nerves, in the case of the kidney this is not so; the kidneys are excited to excrete only when the blood is charged with "harnfähigen" substances. But while this is so, one of the contributions which made him famous was his investigation on the influence of the nervous system on glandular activity. By his classical researches on the submaxillary gland, "Neuen Versuche über d. Beihilfe d. Nerven z. Speichelabsonderung," Ludwig completely retransformed the theory of salivary secretion, and of secretion in general. He showed that secretion could no longer be regarded merely as a process of filtration; on the contrary, it was associated with chemical and thermal changes in the gland and in the

blood streaming through it. Nay, he showed that secretion might take place in a decapitated animal, provided the nerve going to this gland be stimulated. Numbers of his pupils continued to work at salivary secretion, both from its physiological aspect, *e.g.*, Becher, Rahn, Spiess, and its histological.

Indeed Ludwig's contributions to the physiology of the excretions may be regarded as amongst his most important contributions. In connection with the salivary glands, his histological investigation paved the way for important experimental results. With Gianuzzi he investigated the structure of the salivary glands, and in his celebrated paper, "Ueber den Einfluss d. Beschleunigung d. Blutsstromes auf die Absonderung des Speichels," Gianuzzi discovered those curious bodies in the mucous salivary glands which still bear his name and are called "demilunes" or "Gianuzzi's crescents". There are numerous and classical investigations on the secretion of saliva. Indeed so late as 1889 we find Novi investigating "Die Scheidekraft d. Unterkiefer drüse". But while Ludwig's investigations not only completely transformed the theory of secretion, by his investigations he added an entirely new class of nerves to those already known, *viz.*, those known now as "secretory nerves".

Up to the time that Ludwig stimulated the nerves to the salivary glands, physiologists were acquainted only with contraction of muscle resulting from stimulation of the peripheral end of a divided nerve. That the nervous system affected the secretions and excretions had been known from time immemorial—for example, the flow of tears under emotion, the arrest of salivary secretion under the influence of fear—but these results might be due to the supply of blood or other cause. When Ludwig began his researches, all these and other secretions were regarded merely as due to simple filtration of fluids through different membranes. Ludwig proved that this is not so, but that the nerves act directly upon the gland cells themselves, and thus regulate the chemical metabolic processes going on within the secretory cells. Ludwig's great contemporary, Claude Bernard, discovered the "vaso-motor nerves," or those nerves

which influence the calibre more especially of the small arteries. Could the physiology of salivary secretion be explained by this new mechanism? Ludwig by ingenious experiments soon showed that in salivary secretion there was another influence at work beside the action of vaso-motor nerves, *viz.*, the action of glandular or secretory nerves, so that one of his great discoveries was the existence of a separate class of nerves now known as "secretory nerves".

But perhaps Ludwig's greatest interest was the doctrine of the source and movements of the various juices of the body, more especially of the blood and lymph, and here we have to deal with an amazing number of contributions. Indeed, in no other laboratory have the phenomena connected with the movements of these fluids been so thoroughly investigated as in that of Ludwig. The arrangement of the muscular fibres of the heart early attracted his attention, but to him this meant more than merely to describe their arrangement. He sought to explain this arrangement, and how it was useful to the heart to enable it to empty itself, why also the right ventricle forms a crescent-like cavity round the left ventricle, while by a study of the arrangement of the fibres around the auriculo-ventricular orifices he showed how important it was from the point of view of preventing the reflux of blood into the auricles during systole that these orifices contracted, thus affording at the same time a better *point d'appui* for the segments of the auriculo-ventricular valves, the arterial orifices at the same time remaining unchanged in their dimensions.

Ludwig's name, however, will remain indissolubly connected with his epoch-making invention of his "Kymograph" or "Kymographion" or "Wave-writer," an instrument to be found in all laboratories. The importance of this invention is twofold: first, on account of its practical value as a means of measuring the pressure exerted by the blood upon the inner surface of the larger blood-vessels, *i.e.*, the blood pressure, but second, because by it Ludwig founded the graphic method in physiology, a method

which had previously been adopted for other purposes by our distinguished countryman, Thomas Young, but which had been forgotten, so that Ludwig not only founded the graphic method, but gave it back in a greatly improved form both to physics and meteorology.

When an artery is severed, as every one knows, the blood comes out in jets, a proof that the pressure within the arteries exceeds the atmospheric pressure. The Rev. Stephen Hales, Rector of Teddington, described in 1727 in his *Statical Essays* how he introduced the end of a long vertically placed tube, seven to eight feet in height, into the femoral artery of a mare, and observed how high the blood mounted in this tube. This it did to a height of nearly seven feet, so that this represented the "blood pressure". In 1828, Poiseuille, a French physician, connected a blood-vessel of an animal with a U shaped glass tube, or manometer, partly filled with mercury, one end being connected to the artery by means of a lead tube filled with carbonate of soda solution to prevent the blood from coagulating. Thus any oscillations of the mercury could be read off in the open limb of the tube. This was a great advance on the method of Hales, but still it was unsatisfactory. Ludwig, in 1847, modified this instrument by placing a light swimmer in the open limb of the tube and causing the swimmer to record its movements on a revolving surface moved by clockwork. By this method a permanent graphic record was obtained, a curve whose height was an expression of the blood pressure within the artery, and its extent a measure of the time of the several phases of the curve. For the first time the pulsations of the living heart were recorded on paper.

The story of the Kymograph is an interesting one. In 1846, while Ludwig was still in Marburg, he studied the relation between the respiratory movements and the blood pressure, and on his Kymographion he recorded the movements of respiration and the variations of the blood pressure simultaneously. As it were by inspiration the graphic method was founded. For the first time on the same piece of paper was recorded the respiratory movements of the

chest-wall, the rapid movements of the heart, and the blood pressure. Thus was opened up a new and fertile field of study. Since then in all sciences the graphic method has played an essential part. On its applications to physiology, Marey, Professor in the Collège de France at Paris, has written a whole book, and he himself has greatly extended our knowledge of technical appliances for this purpose. Ludwig published his results in Müller's *Archiv*, pp. 240-302, 1847, "Beiträge zur Kenntniss d. Einflusses d. Respirations-Bewegungen a. d. Blutlauf im Aortensystem".

To a dearly loved and much esteemed pupil, Professor Angelo Mosso of Turin, Ludwig presented the original tracing which was published in Müller's *Archiv* with the characteristic dedication on the back of the tracing, written in the neat crisp characters for which Ludwig was famous, "Der Sammlung des Freundes Mosso stiftet dieses erste Stammeln des Herzens und der Brust. C. Ludwig, Leipzig, 15th August, 1874." On the front of the tracing was the date xii., 1846. It is obvious that the Kymograph, apart from its intrinsic importance in the laboratory, is the forerunner of the innumerable applications of the graphic method. To record one tithe of the results won by this instrument or its descendants would be to record the progress of the physiology of the vascular system during the last half-century.

Confining our attention for the present to the heart, in vol. i. of the *Arbeiten*, issued in 1867, we have a description of a very remarkable nerve. In the neck of the rabbit and some other animals there is a small isolated nerve, a branch of the vagus nerve in the neck, which pursues an independent course and runs to the heart, or, speaking physiologically, proceeds from the heart, to join the vagus, and is carried above the larynx in this nerve-trunk to join the medulla oblongata or bulb. This nerve was discovered by Ludwig and Cyon, and was named by them the "depressor" nerve. Why? They found that when this nerve was divided and its *central* end stimulated, the blood pressure in an artery being recorded simultaneously by means of the Kymograph just described, after a few seconds there was a

steady fall of the blood pressure within the arterial system. Here we have a very remarkable phenomenon, how is it to be explained? Well, this nerve is an afferent nerve from the heart, *i.e.*, it conducts impulses from the heart to a part of the central nervous system or bulb, which, when they reach that organ, so modify the activity of the so-called vaso-motor centre situated therein, as to permit of the blood-vessels of the abdomen becoming so dilated that so much blood accumulates within them as to leave only a limited amount for the general circulation, and hence the steady fall of the blood pressure. But what is the use of this nerve? Ludwig suggested that by means of it the resistance offered to the heart, the essential motor-factor in the circulation, was regulated. In other words, when there is too great resistance to the heart, so that it cannot with ease discharge its contents, the terminations of this nerve in the heart are excited and thus lead to a dilatation of the abdominal vessels, whereby the resistance in the arteries is diminished. Thus through this nerve the heart —within limits—can regulate the resistance offered to the blood discharged from it.

This nerve is "depressor," therefore, because when stimulated it brings about a diminution of the pressure within the systemic arteries. It is, however, also the sensory nerve of the heart, and is, probably, one of the nerves concerned in the excessively painful attacks which characterise "angina pectoris" or breast-pang. At the present time this nerve may have a special interest apart from its physiological significance. Few of our readers will, perhaps, recognise in the co-discoverer of this nerve, and one of the earliest pupils of Ludwig, a man who has played many parts in the world—physiological and political. Cyon, as his career has proved, was what the Germans call a many-sided man. The writer made his acquaintance in Leipzig many years ago, when he was Professor of Physiology in St. Petersburg, and, if he is not mistaken, the M. de Cyon who a few weeks ago was deprived of his rights as a Russian citizen by an Imperial ukase is E. Cyon of depressor nerve fame. Many years

ago Cyon left St. Petersburg, and devoted himself to journalistic work in Paris.

Purposely not taking Ludwig's discoveries in chronological order, let us turn for a moment to another nerve in connection with the heart, whose action he discovered, *viz.*, the nerve which from its action he called "Accelerans Cordis". This nerve passes from the upper dorsal region of the spinal cord to the heart, and when it is divided and its peripheral end stimulated, it causes a great acceleration in the number of heart beats, hence the name given to it by Ludwig and Baxt. In this country it is also called Augmentor cordis. Naturally, Ludwig compared the effect of the vagus on the heart, discovered by his predecessor E. H. Weber, with that of the accelerans. The vagus or its inferior cardiac branch when stimulated slows or may even arrest the action of the heart in the relaxed or diastolic phase for a few seconds. On comparing the results obtained by simultaneous stimulation of these two nerves, Ludwig and Baxt came to the conclusion that they do not act on the same intracardiac mechanism.

But while the Kymograph enabled Ludwig and his pupils to discover numerous important facts relative to the action of the heart, it primarily put one in possession of facts which led to an entirely new conception of the activity of the blood-vessels or at least the action of the nervous system thereon. Ludwig and Thiry, in 1864, showed the influence of the cervical spinal cord on the blood stream, dwelling particularly on the fact that the varying and variable capacity of the portal, *i.e.*, abdominal, area was largely concerned in the regulation of the blood pressure in the branches of the aorta, a fact which fits in well with the subsequent discovery of the depressor nerve.

In the medulla oblongata or bulb, Ludwig and his pupils, Owsjannikow and C. Dittmar, succeeded in determining accurately the position of the so-called chief vasomotor centre, *i.e.*, a part of the nervous system from which impulses are continually proceeding to the smooth muscular fibres, more especially of the small arteries or arterioles, whereby these muscular fibres are kept in a state of partial

contraction. The effect on the blood-flow, and, therefore, on the nutrition of the part supplied by such an arteriole, is obvious. This partial contraction is called vascular tonus.

Ludwig and his pupils mapped out accurately the position of the vaso-motor centre. To Claude Bernard belongs the merit of the discovery of the nerves which influence the calibre of blood-vessels. On dividing the sympathetic nerve in the neck of a rabbit he found that the blood-vessels of the ear and face on that side flushed or rather blushed, while stimulation of the distal end of the nerve caused pallor of the corresponding parts.

Perhaps one of the most important methods introduced by Ludwig, but which grew gradually from small yet important beginnings, is that of the nutrition of excised organs. This method consists in circulating or perfusing blood or other fluid through the blood-vessels of an excised or surviving organ, *i.e.*, when the excised organ possesses blood-vessels. The fluid perfused may be blood or other fluid. The method had its origin in the experiments conducted by Cyon and also by Coats on the "isolated," *i.e.*, "excised," frog's heart, and gradually it grew in importance until it came to be one of the most important methods used in studying the localised action of drugs.

This method depends on the fact that all our tissues or organs do not die simultaneously. After so-called death of the individual, the separate organs, especially in cold-blooded animals, may retain their vitality for many hours or even days. The heart of a frog or tortoise when cut out of the body will beat under appropriate conditions for one or two days. By means of this method the chemical and other conditions necessary for cardiac activity were studied. In studying the action of "surviving excised organs," *e.g.*, the frog's heart placed in connection with a recording manometer, H. P. Bowditch, Professor of Physiology in Harvard University, Boston (U.S.), showed that the muscle of a frog's heart, quite independently of the strength of the stimulus applied to it, either gave a maximal contraction or it did not contract at all. On this A. Mosso, with that Southern poetic spirit which characterises his way of look-

ing at physiological facts, remarks "that the heart is at once the most delicate and strongest of all our organs, because when excited by weak stimuli, it always responds with a strong contraction—in fact, with its strongest possible response" (*Die Nation*, 29th June, 1895).

As regards the heart and the cause of its rhythm, quite a number of the older theories regarding its rhythmical action have been put aside, largely owing to the results obtained by this method. It is, at least, something to get rid of false theories.

Keeping still to this method of perfusion, it has had a far-reaching influence. The writer can well remember with what diligence Luciani, now Professor of Physiology in Rome, perfused blood through the excised heart of a frog, and studied the remarkable groups of beats, with intervening pauses, which bear his name of "Luciani's groups".

In the opinion of the writer about this time there commenced a movement of young men from Italy and Leipzig which has exerted a great, not to say commanding, influence on the physiological achievements of our Italian *confrères*.

Under the guidance of Kronecker, then the assistant of Ludwig in the experimental department, Luciani studied with patience the remarkable periodicity in the action of the frog's heart whereby groups of beats alternate with periods of repose.

The artificial circulation of blood through excised organs was obviously capable of great expansion. Ludwig proposed to A. Mosso to study artificial circulation in an excised kidney, and the writer remembers what lively interest was awakened in the laboratory by the earliest experiments, *i.e.*, to study the life of a kidney after death. It was soon discovered that even twenty-four hours after the kidney was excised from the body, this organ still lived or survived, or at least that its blood-vessels did, and responded to the action of drugs.

From these investigations originated the idea suggested by Ludwig to Mosso of measuring the volume of an organ due to variations in the size of its blood-vessels. Mosso

arranged an apparatus by means of which variations in the volume of a limb or other organ might be easily recorded whilst still in connection with the living animal. This he called a plethysmograph. Very many investigations have been made by it. By means of it one can show that the volume of the arm, or a limb, varies with the activity of the brain, and over and over again has the writer seen the effect of a mental effort—*e.g.*, an arithmetical question while being solved by the person experimented on—varying the amount of blood supplied to the arm, and therefore the volume of the limb. Many other Italian pupils flocked to Leipzig—for Ludwig had always profound sympathy with his Italian "young friends". Perhaps it will not be taken amiss by his Italian *confrères* and friends if the writer ventures to express his opinion that the extraordinary activity in physiological circles manifested by the younger Italian pyhsiologists is largely if not directly due to the inspiration of Ludwig.

As Italian pupils, we may mention Ceradini (1871), perhaps one of the most talented amongst his compatriots. Alas, that Ceradini should have turned his attention to other affairs! His research on the semi-lunar valves is one of the best monographs on this subject, and still remains a classical memory. Gianuzzi, who has given his name to the "crescents" which occur in the acini of certain mucous salivary glands, and then followed Luciani, Mosso, Gaglio, Baldi, Grandis, Fano, Martinotti, and many other well-known Italian investigators.

By a comparatively simple method Ludwig showed the connection between serous cavities such as the peritoneum and the lymphatics, and demonstrated how the respiratory movements aid in the passage of lymph and even particulate bodies from the peritoneal and pleural cavities into the lymphatics *viâ* the stomata. With his first assistant in his histological department, Schweigger Seidel, who died early, he invented the now familiar method of injecting lymphatics by the "Einstich" or "Puncture" method. Brücke had discovered the value of soluble Berlin blue as an injection mass, but it remained for Ludwig, reasoning from the

presence of the valves in the lymphatics, to conclude that the natural path for the lymph and therefore other fluids to pass in these vessels was from periphery to centre, and so he hit upon the apparently simple yet really scientific method of injecting these vessels with a watery solution of Berlin blue, the nozzle of a fine hypodermic syringe being thrust under or into the skin or other organ, when lymphatics are immediately filled with the blue fluid. Indeed it is a most beautiful and instructive experiment to show the lymphatics in the limb of a dead animal by this method. In this way these observers were able to study the distribution of lymphatics in fasciæ and tendons, and to show how important are movements for the onward flow of the lymph, a view that was confirmed by the experiments of Genersich, Lesser, Emminghaus, and others on the effect of movements —active and passive—on lymph flow. Ludwig was continually striving to arrive at a correct knowledge of the relation of the tissue elements to the lymphatics on the one hand, and the blood-vessels on the other. Numerous experiments led him to formulate the filtration hypothesis regarding the formation of lymph, an hypothesis which is still *sub judice*, and is even to-day the subject of lively controversy. Whatever theory be adopted Ludwig's facts remain.

During the last ten or twelve years, Ludwig's pupils experimented largely on the process of Nutrition as manifested in the absorption and fate of proteids, sugars and fats. Numerous are the investigations on the course of the fat on its way to the blood. With what care and exactness did Ludwig operate when exposing the termination of the thoracic duct! Indeed he was an ideal operator, and had he chosen to turn his attention to surgery few would have equalled him in the art. The passage of proteid and sugar into the blood was the subject of many investigations, and so was the effect of the injection of so-called peptones into the blood stream (Schmidt-Mulheim and Fano) whereby the blood of the dog when it was shed was prevented from coagulating. Correlated with this are the numerous investigations on the respiratory processes involving a study of the gaseous exchanges in the lungs and the tissues.

To Ludwig we owe our earliest exact knowledge of the blood gases, and to this end he devised much apparatus which is in daily use in physiological laboratories. A question which always had great interest for him was the respiration of the tissues, whether oxidation took place in the blood or in the tissues, a question now answered in the affirmative for the tissues. There are many researches on the physiology of muscle, including the famous work of Kronecker on its fatigue and recovery.

The physiology of the spinal cord and the doctrine of reflex action was thoroughly studied, both in the frog and mammal. Our knowledge of the sensory paths in the cord was greatly extended by the researches carried on in his laboratory, while Ludwig himself devised a whole series of delicate instruments for section of limited parts of the cord. It was when Flechsig was his assistant in the histological department that, under Ludwig's inspiration, he made those fundamental investigations on the medullation of nerve-fibres, which enabled him to map out new conducting paths in the cord. This research at once established Flechsig's fame, and led to his appointment to the Professorship of Psychology in Leipzig. Indeed by a curious coincidence Flechsig was Rector of the University at his master's death. In his speech at Ludwig's grave as Rector, Flechsig remarked that "Ludwig was not only a high priest of his science, but of science in general".

There is not a large number of investigations on the sense-organs, while on the brain or rather cerebrum and cerebellum I do not recollect to have seen any independent investigations.

Numerous investigations of a more or less chemical nature were carried on, for obviously experimental and chemical physiology must frequently go hand in hand, and in this respect Ludwig was fortunate in having Drechsel as his assistant, and in later years Siegfried. Another of Ludwig's pupils whose contributions belong to the borderland between chemistry and pharmacology is Schmiedeberg, the distinguished and genial Professor of Pharmacology in Strassburg.

Ludwig always selected his assistants with great judgment and discrimination. In the histological department, he had successively Schweigger Seidel, Schwalbe, Flechsig, Gaule, Drasch and Moser. In the physical or as he called it experimental department, Kronecker, von Kries and von Frey, while Drechsel was his chemical assistant until called to Berne as Professor of Physiological Chemistry. Those of us who had the great good fortune to study under Ludwig when Hugo Kronecker, now Professor of Physiology in Berne, was assistant, can never forget how much we owe to Kronecker, who was a second *genius loci* and the fast friend and helper of us all. He it was who led the way to the foundation of the then "physiologische Gesellschaft" in Leipzig, and who was the originator and presiding genius at the "functions" that followed after the formal communication of "papers" at the evening meeting of the society. But Ludwig's personal assistant, in the narrower sense of the word, was his mechanic Salvenmoser, who accompanied him from Vienna. He was simply invaluable both to professor and pupil, and he always did his work so thoroughly, yet quietly. With the death of his master Salvenmoser has retired, partly on a pension or, rather, on an annuity subscribed for him several years ago by Ludwig's pupils.

One other feature deserves notice, *viz.*, Ludwig's care for animals operated on. He lays it down explicitly that an operation on a living animal should only be undertaken after careful forethought of all details of the experiment, and after the animal has been rendered insensible by the use of some anæsthetic. He would never allow an animal to be used for a second operation. In 1879-80 he was President of the Society for the Protection of Animals in Leipzig, and remained a member of this society until his death.

Such is a rapid sketch of the work which was conducted under Ludwig's direction, and it will be obvious that Ludwig was an "all-round" physiologist, and perhaps the only one who could be compared with him in the breadth and variety of his knowledge was his former colleague and friend, Brücke,

who, like Ludwig, was also deeply imbued with the artistic sense.

It is, perhaps, not too much to say that Ludwig's labours have largely revolutionised physiology, while our whole manner of looking at physiological problems is largely due to the influence he has exerted on the progress of the science. He was essentially, and above all, a born experimenter. He was thoroughly at home both in the laboratory and lecture room. His views he always submitted to experimental investigation, and in doing so he exhibited a faculty for the invention of mechanical appliances which was surprising. One, perhaps, of the best known and most ingenious of these is his method—along with Dogiel—of estimating the velocity of blood-flow by means of his "Stromuhr," an instrument well known to students of physiology. Ludwig was, in the truest and widest sense of the word, not only a physiologist but also a philosopher. It was not enough for him to "speculate" or "theorise" upon a subject, the theory had to be submitted to experimental proof.

Moreover many departments of physiology which had long remained within the sphere of vitalistic influence were by him submitted to investigation by the most exact physical methods. In fact Ludwig did for physiology as a whole, what Spallanzani did for a great chapter of chemical physiology. Abbé and professor were imbued with the same spirit.

The preceding pages are to be regarded as a slight tribute of admiration and gratitude, and as a thankoffering to C. Ludwig, that "homo ammirabile" as Fano calls him, from one who received much kindness and instruction at his hands, and who had the honour and privilege of much intercourse with him for many years. Whatever monument may be raised to his memory by his pupils, his works and the imperishable impress which his genius and his labour have imprinted on physiology will ever remain his true monument, to attest the greatness of his intellect to succeeding generations.

WILLIAM STIRLING.

ON THE CHEMICAL NATURE OF ALLOYS.

IN the preceding article I assumed for the sake of simplicity that no chemical action takes place between the metals of an alloy. The evidence on this point is very imperfect, but in the majority of cases the balance of probability is in favour of this view. Some metals, however, undeniably combine in definite proportions to form chemical compounds.

It has been already mentioned that the depression in the freezing point of a solvent is proportional to the number of molecular weights of foreign substance dissolved in it. Moreover, the depression predicted by Van't Hoff's law as due to one *molecular* weight, is in many cases produced by one *atomic* weight of a metal when dissolved in other metals. Hence it seems probable that a metal is separated into individual atoms during the process of solution. It may be, however, that molecules are formed containing an atom of the dissolved body associated with one or more atoms of the solvent. Where the depression in the freezing point is persistently one half the theoretical, as in the case of aluminium dissolved in tin and of mercury or bismuth in lead, it becomes, at least, probable that the molecules in solution are Al_2, Hg_2, Bi_2 respectively. Tin in lead appears to form molecules of the type Sn_4.

In these cases we do not know how much, if any, of the solvent is associated with the molecules of the dissolved metal. In fact the cryoscopic study of binary alloys throws very little light on the question of chemical combination; but when we examine ternary alloys we get more evidence on this point.[1] For, as a general rule, when two metals B and C are simultaneously dissolved in a third, the total depression in the freezing point is the sum of the effects that B and C separately would produce. This fact indicates that the atoms of B and C have not combined with each

[1] Heycock and Neville, *Chem. Soc. Jour.*, 1891, p. 936, and 1894, p. 65.

other, for such combination would decrease the number of molecules in solution, and consequently diminish the depression in the freezing point. When, however, one of the metals tin, bismuth, thallium or lead is the solvent, and gold and cadmium are added in succession, we obtain evidence of combination between the two latter. For if the gold be present in sufficient quantity, and the cadmium be now added, then, instead of causing a further depression in the freezing point, this latter metal will bring about a rise. Successive additions of small quantities of cadmium will produce alloys whose freezing point gets higher and higher until an equal number of atomic weights of cadmium and gold is present. With such a proportion, the alloy has a maximum freezing point, which is still, however, lower than that of the pure solvent. This maximum freezing point is quite independent of the amount of gold and cadmium present, so long as this exceeds a certain value, and the two metals are in equal atomic proportions; but an addition of either gold or cadmium beyond this proportion produces a further fall.

These phenomena point to the conclusion that the gold and cadmium have partially combined to form molecules, AuCd, of a body which is less soluble than either metal. This compound has been separated from the mother liquor in a somewhat impure form by a process of filtration; it has also been prepared in a purer form by alloying gold with cadmium and removing the excess of the latter metal by distillation in vacuo.

When aluminium is dissolved in tin and gold is added a quite insoluble body $AuAl_2$ is precipitated. Gold with zinc, and silver with cadmium or zinc, probably form similar definite compounds, but they have not been so thoroughly studied.

If the above view as to the nature of these alloys be correct, we see a selective power of combination with each other shown by the two metals in solution. The solvent metal, though present in overwhelmingly larger proportion, does not appear to appropriate either of the others; but the evidence we possess does not tell us whether the molecule

of the compound in solution contains atoms of the solvent metal or not. All we can say is, that the gold and the cadmium behave in the same way to each other whether the solvent metal be tin, thallium, bismuth or lead. This fact makes it probable that the chemical influence of the solvent is a subordinate one as compared to the action of the two metals on each other.

It is noteworthy that the compounds of gold or silver with cadmium do not appear able to exist in solution except in the presence of excess of the constituent metals. In this respect they resemble many chemical compounds that partially dissociate in solution. We can arrive at this conclusion in the following way. The total depression in the freezing point of the alloy below that of the pure solvent gives us, by Van't Hoff's law, the total number of molecules in solution, irrespectively of whether these molecules are complex or simple. But we know the weights of gold and cadmium that have been added, and combining these several data we can calculate the concentrations, x, y, z, of free gold, free cadmium, and of the compound AuCd which are in solution. The calculated values show that although x, y, and z vary very much for different alloys, yet they are connected by the law usual in cases of dissociation, namely, that when x is large y is small, and *vice versâ*; or, more exactly, that their values obey the law of mass action $xy = kz$, where k is a constant. The compound of gold and aluminium is more stable, and does not dissociate when in solution in tin.

There can be no doubt that further cryoscopic examination of ternary alloys would bring to light numerous cases similar to the above.

Another method of research has been adopted by Laurie,[1] who has studied the copper-zinc, copper-tin, and gold-tin alloys, and as the methods and results are similar in the three cases, we will consider the experiments on copper-tin. He prepares a series of alloys containing increasing percentages of tin, and makes each alloy in turn one plate of

[1] *Chem. Soc. Jour.*, l., p. 104, 1888; and *Phil. Mag.* [5], 33, p. 94.

a voltaic cell, the other plate being copper, and the electrolyte being a solution of stannous chloride in hydrochloric acid. The electromotive force of each combination is then determined and the results plotted. The curve thus obtained shows that so long as the alloy contains less than 40 per cent. of tin, the electromotive force is small, somewhat irregular, and does not vary much from ·1 volts, but that as soon as this percentage is exceeded, it runs up to a much larger and very constant value of ·56 volts, a value which is independent of the excess of tin present. The change probably occurs with a slightly smaller percentage of tin, very near the composition Cu 61·64 per cent., which corresponds to the formula Cu_3Sn. He considers that the change is due to the first appearance of crystals of free tin in the solid alloy, the tin when in less quantity being all in combination with the copper as Cu_3Sn. Of the other body Cu_4Sn, which Riche believes to exist, he found no indication.

In the copper-zinc alloys a similar change in electromotive force was found to occur when the composition $CuZn_2$ was reached, and in the gold-tin series at about AuSn. These experiments belong to a large class, including those of Matthiessen, Oliver Lodge, Wiedemann, and others on electrical conductivity, those of Roberts Austen with the induction balance, and numerous experiments on the density of alloys. In all these the physical properties of a series of alloys have been shown to experience an abrupt change at a particular composition. That these facts point to real and important differences between the alloys on either side of the critical percentage is certain, but it would be premature to conclude that their origin lies in chemical combination. Other explanations certainly can be offered. For example, it has been suggested that the much finer grain in the crystalline structure of alloys between certain percentages might prevent the electrolyte from attacking the more positive metal except superficially, and so cause the electromotive force after a very short time to become that of the more negative metal.[1]

[1] Foerster, *Naturwissenschaftliche Rundschau*, ix., 36.

Mylius and Fromm[1] have made an interesting series of experiments on the formation of alloys by precipitation out of aqueous solution. They find that when a plate of zinc, cadmium, or tin is introduced into a very dilute solution of a salt of a less positive metal, the dark flocculent precipitate which is formed on the surface of the plate contains not only the metal which was in solution, but also a considerable percentage of the more positive metal of which the plate is made. Thus zinc, introduced into a solution of copper sulphate, causes a precipitate which may contain as much as 50 per cent. of zinc, and which, when compressed and burnished, is evidently *brass*. Their explanation of the phenomenon is that at a spot where the copper has been electrolytically deposited and wholly removed from the liquid, it finds itself surrounded by a solution of zinc sulphate, so that, with the neighbouring zinc, a couple consisting of zinc, zinc sulphate, and copper is formed. Consequently a thin layer of zinc will be deposited on the copper. This would at once stop the action but for the diffusion of the zinc into the copper as an alloy. The surface of zinc being thus converted into one of alloy, more zinc will be deposited until the copper is saturated with zinc, that is, until the electromotive force of the couple zinc, zinc sulphate, alloy is zero. Laurie's work would lead us to expect that the electromotive force would vanish when the alloy had a definite atomic composition, and Mylius and Fromm found that this was sometimes, but not always, the case. The precipitates of zinc-silver, zinc-copper, and cadmium-silver do not appear to have a definite composition, but cadmium precipitates out of a 1 per cent. solution of copper sulphate a body whose composition corresponds pretty nearly to the formula Cu_2Cd. They found that cadmium in a dilute solution of gold trichloride forms an alloy which has exactly the composition $AuCd_3$, and they remark on the extreme readiness with which gold and cadmium combine. Perhaps the most interesting alloy formed by them is the body Cu_3Sn, which grows in a crystalline form on a plate of tin when immersed in dilute

[1] *Berichte d. d. chem. Gesellschaft*, xxvii., 630.

copper chloride or sulphate. This is Laurie's compound, and they draw attention to the fact that many observers have pointed out this alloy as possessing remarkable properties. The alloy Cu_3Sn has the greatest density of all copper-tin alloys, it does not liquate, and it is the best conductor of heat and electricity. It is difficult to deny its right to be regarded as a chemical compound.

Mylius and Fromm have made many experiments in addition to the above, and indeed have opened up a new field of research in which much fruit will no doubt be gathered.

Purely chemical methods have been employed very successfully by Deville and Debray in their study of the definite compounds formed by zinc or tin with some of the metals of the platinum group.[1] They found that palladium, platinum, rhodium, iridium, and ruthenium, if dissolved in a large excess of zinc, produce alloys that, treated with dilute hydrochloric acid, do not yield up the whole of the zinc. A considerable proportion remains apparently in combination with the noble metal. In the case of rhodium, iridium, and ruthenium there is a very large evolution of heat during their solution in zinc. The residual alloys after treatment with hydrochloric acid leave residues possessing a remarkable property. These residues, well washed and dried at 100° C., were heated in vacuo to 300° C., when there was a change of state accompanied by a large evolution of heat. We see here indications of chemical action even when alloys of definite composition have not been found.

Debray[2] has also studied the alloys produced by dissolving the above metals in an excess of tin, and in this process, as before, a large evolution of heat was observed. The ingot was afterwards slowly acted on by dilute hydrochloric acid. In the case of the platinum-tin, as the solution proceeded, brilliant plates were observed to stand out from the surface of the mass. These could be easily detached and analysed, and were found to have very exactly the formula $PtSn_4$. By a similar method crystals of the bodies $RhSn_3$

[1] *Comptes Rendus*, p. 1557, 1882.
[2] *Ibid.*, p. 1470, 1887.

and $RuSn_3$ were obtained. When osmium was dissolved in zinc or tin, and the alloy treated with hydrochloric acid, a residue of pure osmium was left, so that osmium appears to dissolve in these metals without combining with them. When lead or copper were used as solvents, similar evidence of chemical combination was obtained.

Heycock and Neville have found that when gold is dissolved in sodium, and the apparently homogeneous alloy is treated with alcohol, and finally with water, a beautifully crystalline mass is left consisting of lustrous microscopic needles. These are of a pale brassy gold colour, but when heated to about 200° C. they become incandescent and their colour changes to that of pure red gold. After ignition, but not before, the residual sodium can be extracted with water and estimated. The percentage of sodium agrees pretty well with the formula Au_3Na, but there is never quite enough sodium, probably because the solvent used to remove the excess of that metal attacks the crystals superficially.

By dissolving titanium in aluminium and extracting with hydrochloric acid, Levy[1] has obtained fine crystalline plates which appear to be a mixture of $TiAl_4$ and $TiSi_4$.

Joannis,[2] by treating the metals lead, bismuth or antimony with sodammonium or potassammonium, and removing the excess of alkali metal by washing with liquid ammonia, has obtained the bodies PbK, $BiNa_3$, $SbNa_3$, $PbNa_2$, analysis giving almost exactly the percentage corresponding to these formulæ.

These examples by no means exhaust the list of definite compounds that are known or suspected to exist. But although the evidence in favour of chemical combination is pretty conclusive in some of the above cases, it is well to remember that the original ingot of alloy may consist of crystals of the solvent metal imbedded in eutectic alloy, and that this latter, though not a chemical compound, may be such an intimate mixture of the two metals that reagents can only attack it superficially. We should then get a

[1] *Comptes Rendus*, p. 66, 1888.

[2] *Ibid.*, p. 585, 1892.

residue of constant composition which was not a chemical compound.

The microscope promises to become as important to the student of metallurgy as it has long been to the geologist, the connection between the mechanical properties of an alloy and its microscopic structure being a very close one. The work of Osmond and others on the microscopic structure of steel deals with a material too complex for adequate discussion here, and of the valuable treatise by Behrens on the minute structure of alloys, the same must be said. However the results obtained by the last-named writer, by Guillemin,[1] and by Charpy,[2] present certain general features. Guillemin etches the polished surface of a brass or bronze with dilute sulphuric acid; the etched surface is then examined microscopically, and the image photographed. Different alloys give very different surfaces, but the same alloy gives a surface of constant character. It presents more or less twisted furrows separated by parts standing out which the acid has spared. He says "it is certain that at the moment of solidification the metal undergoes a liquation, and separates into several alloys of definite composition, which are unequally attacked by the acid".

Charpy applies Guillemin's method, with modifications, to the study of brass, and concludes that the brass is formed of two alloys, one being crystalline and the other amorphous. When the alloy is heated the crystals begin to lose their shape at 700°, and at a rather higher temperature zinc volatilises from them. He utilises electrical action to bring about the etching, making the brass surface to be etched the zincode of a Daniell's cell. This method of effecting fractional solution appears a promising one.

Behrens in his work on *Das Mikroskopische Gefüge der Metalle und Legierungen* gives a vast amount of information on this subject. He etches either the natural surface or a polished section of the metal with a variety of solvents, and also brings out texture by partial oxidation produced by heat. The appearance of structure in the alloys he examined

[1] *Comptes Rendus*, 115, p. 232, 1892.
[2] *Ibid.*, 116, p. 1131, 1893.

is almost universal. A few alloys, such as Levol's eutectic copper-silver alloy, Cu_2Ag_3, and speculum bronze, $SnCu_3$, which Riche has shown to be free from a tendency to liquate, show no structure under the microscope; but all others can be made by etching, or surface oxidation, to show two constituents. This is very well seen in copper-silver alloys. In pure copper the boundary lines between the crystals can with difficulty be detected, but the addition of even a tenth of a per cent. of silver produces a network of slender white lines between the faces of the crystals of copper. These lines grow in width with increasing percentage of silver, and when the alloy attains the composition of the eutectic alloy, about Cu_2Ag_3, they form the whole surface, the crystals of copper having disappeared. With a larger percentage of silver, dendritic crystals of this metal can be seen, *so that in all mixtures of these two metals we find an alloy of mean composition filling up spaces between crystals of the metal present in excess.*

Exactly similar phenomena are shown by bronzes, and by many other alloys. It is hardly necessary to point out that these facts agree very well with the views expressed in the previous pages. Many of Behrens' results, however, suggest the conclusion that the proximate constituents of a solid alloy are frequently themselves alloys, and not the pure metals. As an example of the complexity of the phenomena observed by him, we give here his account of the microscopic structure of a sample of gun metal. "Ten per cent. bronze cast in bars. Well formed rectangular rosettes composed of cubes of 100μ in diameter. The threads of light yellow alloy rich in tin, and measuring 20μ, often have an almost white central line; moreover, most of the pale red cubes are surrounded by a narrow white edge, which stands out plainly from the pale yellow. Spots of tin were not present."

Steel, the most important of all alloys, will probably in the end be found to have a structure similar to the above described. Osmond finds nuclei of pure iron imbedded in cells of a substance rich in carbon and silicon, and Sorby, long ago, detected in many kinds of steel a microscopic

structure of plates of pure iron alternating with thinner plates of a carbide of iron. Osmond[1] considers, however, that as many as five crystalline ingredients can be detected in steel, and that iron itself can, by processes of hardening and tempering, be brought into at least three allotropic states. Let us hope that increasing knowledge will simplify and not further complicate the views held on this subject.

It is certain that although the scientific explanation of all the phenomena revealed by the microscopic examination of alloys may not come to us for some time, yet technical workers who use the microscope will find, and are finding, a correlation between microscopic structure and mechanical properties that cannot fail to be of enormous value to the engineer.[2] For example, the microscope throws light on that important section of metallurgy, the use of small admixtures of reducing agents, such as zinc or sodium with copper. It also promises to explain the curious change by which some bronzes are hardened by reheating.

Behrens, in common with many other students of the subject, is disposed to attribute definite chemical formulæ to some of the constituents found in an alloy, especially to those alloys which solidify homogeneously; but the writer inclines to the view that these eutectic alloys, such as Levol's Cu_2Ag_3, and others of the same kind, may be solid solutions and may have no claim to be regarded as chemical compounds. Whether this be so or not, it is certain that the arguments as to definite chemical composition brought forward by each worker from the use of his own methods, must be tested and supported by other methods before they can be generally accepted.

Dr. Foerster, in a valuable series of articles on the "Chemical Nature of Alloys,"[3] to which the present writer is indebted for several references, suggests that alloys of

[1] Méthode Générale pour l'Analyse Micrographique des Aciers au Carbone. *Bull. de la Société d'Encouragement*, x., p. 480, 1895.

[2] Roberts Austen, *Nature*, Aug. 15, 1895.

[3] *Naturwissenschaftliche Rundschau*, 1894.

definite composition, such as AuCd, Cu_3Sn and others, many of which do not obey the ordinary laws of valency, may be held together rather after the fashion of double salts, may in fact be *molecular* and not *atomic* compounds. In this case they may be dissociated when in solution, but like the alums, exist in the solid form. Hence in a dilute solution of copper in tin, the copper may exist, as Heycock and Neville have shown, in the form of monatomic molecules, but yet when the mass solidifies the bodies Cu_4Sn and Cu_3Sn may be formed. It is certainly somewhat difficult to imagine that the electropositive and self-saturating atoms of gold and cadmium can be held together, in the molecule AuCd, by forces similar to those which maintain the equilibrium of a molecule of hydrochloric acid.

Future progress in this subject will probably be the reward of those who bring physical as well as chemical and microscopical methods to bear on the same problem.

F. H. NEVILLE.

ANTHROPOMETRY IN INDIA.

GREAT as are, and have been, the opportunities for anthropological research afforded by our Indian Empire, it can hardly be said that they have been utilised to anything like the extent that might have been the case. It is needless to dwell on the causes of this defect; they are numerous but sufficiently clear; and the parsimony of the Indian Government in matters relating to science is prominent among them.[1] But neither zeal nor industry have been awanting in European officials acting in their private capacity, and vast masses of ethnological material have been collected by them; the department of physical anthropology has, however, been comparatively neglected, and what has been done therein up to a recent date has not been done systematically. Thus, for example, Dr. John Shortt, on the inspiration of Dr. Barnard Davis, so long ago as 1868, published a long series of measurements of the several tribes of the Neilgherry Mountains, and of certain castes in the plain of the Carnatic; but he omitted to ascertain the length and breadth of the head; and his observations, though valuable, are much less so than they might have been.

Here, as generally elsewhere, the earlier attempts to affiliate and classify the races of the country were based on language. History gave very little assistance; in fact the Hindus can hardly be said to have had any history before the first onset of Mussulman conquest. But a rough division based on language was easily arrived at; the Hindus to the north of the Vindhya were recognised as members of the great Indo-European family, and the occupants of the Deccan, with their Dravidian tongues, were supposed to be the descendants of the primitive population, pushed down into the peninsula by the Aryan

[1] Mr. Risley pleads in excuse of the incompleteness of his work in the Punjab the fact that the government of that province was too poor to assist in it.

conquerors. What were the elements and affinities of that primitive population remained, however, very doubtful; and the problem grew more complicated and difficult with a more complete knowledge of the numerous hill, forest, outcast, or servile tribes found not only in Southern and Central India, but even in the far north and north-east. It seemed doubtful whether these could really be the survivals of the organised and powerful peoples who were spoken of in the old Aryan literature as the hostile occupants of Hindustan. A not unplausible idea, supported by the alleged presence of an outlying Dravidian language among the Brahuis of Biluchistan, was that the best parts of India had been conquered from the aborigines long before the Aryan invasion by a Mongoloid or Turanian people, of whom the Brahuis were the rear-guard. The aborigines might have been of some other type, perhaps akin to the Mincopies of Andaman, or of some other negroid stem. But some observers found Mongoloid features in them too. Dr. Shortt, certainly the most careful observer of the physical characteristics in the last generation, reported of the Kurumbas, a very degraded hill tribe in the Neilgherries, that they had "heads short from end to end, with a lofty crown or dome and prominent forehead, wedge-shaped faces, and obtuse facial angles, hollow cheeks, and prominent molar bones, the nose having a deep indentation at the root about $1\frac{3}{4}$ inches in depth (!), which is general". The length of the nose from root to tip he found to be 1·75 inches, the breadth 1·42 nearly; from which we may fairly conjecture that the Kurumbas are, if not platyrhine, at least very nearly so.

The value of these observations is impaired, for our present purpose, by the doubt whether the Kurumbas are not really a degenerate Dravidian tribe, once at a fair level of civilisation, rather than an aboriginal clan; but they are very interesting in relation to the Australioid hypothesis, of which more presently.

Again, with regard to the Yenadis, another outcast tribe of Madras, Shortt speaks of them as having a

Mongolian type of feature, with broad and prominent cheek-bones, a pointed chin, a scanty mustache, no whiskers, and a scanty straggling beard. Of nine skulls which he obtained, he says: "On a cursory view of these there is nothing remarkable among any of them; they are somewhat small and light, and whilst the female skulls are uniform in contour (*i.e.*, I presume, dolichous, and oval or elliptic in the norma verticalis), the males have a great parietal width". Two of the female crania, which he presented to Barnard Davis,[1] are now in the museum of the College of Surgeons; they are small and dolichous (cranial index, 70·9), and the zygomata are by no means wide. In fact it was clearly on the facial aspect and the scanty development of hair that Shortt founded any ideas he may have had of the Mongolian affinities of these outcasts, and these he was unable to put to the test of measurement, which, indeed, at that date had hardly begun to be applied to the face by anthropologists. Width of the cheek-bones is not necessarily accompanied by width of the zygomata, and is, of course, characteristic of other races besides those which we style Mongoloid or Turanian. It is very general among negros and negroids; and in relation to the breadth of the head even that of the zygoma is almost as often great in the black as in the yellow races.

But Huxley soon brought the search-light of his keen observation to bear upon the people of Southern India, with the result that he detected the resemblance existing between them and the Australians, with whose type he was familiar. The small long head, the deep nasal notch, the broad nose, the dark skin, the black wavy hair were characters which warranted him in denominating the Dravidians Australioids.

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[1] *Thesaurus Craniorum*, vol. i., p. 157.

posed to be accompanied by a difference of physical type; and the former were thought to be the descendants of Mongoloid immigrants from the north-east, the latter to be either the remains of an ancient oceanic population, or to have migrated from the north-west from the region where the Brahui had been left behind.

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The ethnology of this part of India—the valleys, roughly speaking, of the Ganges and the middle Indus—had never appeared so difficult as that of the peninsula, though it had its own complexities, and the absence of historical data was almost as deplorable. The prevailing view was that the primitive Aryans, entering the plain of the Indus from the north-west, had gradually spread eastwards, conquering and civilising until they had occupied almost the entire tract. Some, those who made light of colour and complexion as permanent racial characteristics, supposed that the whole population was of Aryan descent more or less pure, and this view was naturally not unwelcome to the educated and patriotic Hindu. Others believed that the twice-born castes, the Brahmans, Kshatriyas and Vaisyas, were truly Aryan, the Sudras, or mass of the population, being descended from the conquered Dasyus, the ancient occupants, more or less leavened, perhaps, by a tincture of the blood of the superior race. It was natural, also, to suppose that, in accordance with the usual rules of migration and conquest, the proportion of the blood of the invaders would be found greatest in the neighbourhood of the scene of their arrival, on the north-western frontier, that is, and in the Punjab; though the distribution would no doubt be modified to some extent by the greater or less attractions of the soil in the several provinces; thus aboriginal tribes were known to have subsisted in force till a late period in the arid hills and plains of Rajputana, and in the forest tracts of Central India, whereas the Doab of the

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Ganges and Jumna had since the dawn of history, and doubtless long before, been the aim and coveted possession of every invading tribe, Iranian or Turanian, Greek, Pathan, Turk or Mongol.

Different theories of caste complicated the subject. Caste means colour, and the prevailing opinion was that of the Hindus themselves, that the origin of castes was in the distinctions of races who differed physically and palpably. Their great multiplicity was ascribed partly to breach of laws of endogamy by cross-breeding. Thus the Ambastha, or physician caste, was supposed to descend from the intermarriage of Brahmans with Vaisyas. On this theory, however, there is a little difficulty in perceiving why a particular cross-breed should have been confined to a particular occupation, and there were those who preferred to assign the origin of caste distinctions to difference of occupation rather than of blood, and who thought the prohibition of exogamy was of later date. There are, indeed, indications that these prohibitions were not so absolute or so strictly adhered to at some remote period as they have been now for many ages; and it will be found that this will furnish an almost necessary explanation of certain general resemblances in the provincial populations.

Mr. Risley, taking his method and instructions from Topinard and Flower, and using chiefly native assistants, who appear to have carried out their work with great intelligence and zeal, ascertained the stature and weight, and the principal kephalic and facial measurements in a considerable number of individuals of each of 87 castes or tribes, of which 15 belonged to the province of Bengal, 5 to Chittagong, 10 to the Darjiling Hills, 10 to Behar, 17 to Chota Nagpur, 23 to Oudh and the north-west provinces, and 9 to the Punjab. In a good many cases he succeeded in getting 100 subjects of each caste, but most of those in the Punjab, and some in the other provinces, are but insufficiently represented. The total number of persons examined was nearly 6000.

The chief inferences which Mr. Risley drew from this

extensive investigation were, first, that the castes are, generally speaking, really ethnological and not merely social divisions; second, that three main types may be distinguished in the population of India at the present day.

1. A leptorhine (long-nosed), pro-opic (*i.e.*, hatchet-faced, or as the Chinese say of us, horse-faced), dolichokephalic type, of tall stature, light build, narrow face, comparatively fair complexion, and high or open facial angle. This type is most marked in the Punjab. Their exogamous groups are eponymous, names of Vedic saints or heroes.

2. A platyrhine, mesopic or nearly platyopic, dolichokephalic type, of low stature, thick-set, with very dark complexion, broad face, and usually low facial angle; most distinct in Chota Nagpur and the central provinces. Its sections are totemistic, names of animals, plants, etc., to all of which some form of taboo applies.

3. A mesorhine, platyopic, brachykephalic type, of lower medium stature, sturdy build, yellowish complexion, broad face, and low facial angle; found along the northern and eastern frontiers of Bengal. Exogamous groups curiously named, mostly by nicknames of supposed founders.

Of these the first is evidently the Aryan type, the type, that is, which has from time to time filtered in from the north-west across the Indus. Except in colour, it recalls the tall blonde type of Northern Europe; except in stature, perhaps, as Topinard suggests, the dark Mediterranean type. The dolichokephaly is probably inherent in the immigrant stock, not acquired by admixture with the black Australioids, for it is as strong among the Punjabis (abstraction having been made of the trans-Sindhian Pathans and Biluchs) as anywhere, and would not, in the Sikhs and Gujars, exceed 71 in the bare skull. It exists, moreover, in an exquisite degree among the north-western mountains, in the Cashmiris, the Dards, and Chitralis, people who, from their position in the direction of the supposed fountains of migration, should be more Aryan than the most

Aryan Hindus, should be in fact to them what the Holsteiner or Frisian is to the Englishman.

These Dards, be it noted, though a few among them may be blonde, seem to be generally dark-eyed and dark-haired; and the same seems to be the case with the Kafirs of the Hindu Kush.

Topinard in his commentary on Risley's tables hints his surprise at the smallness of the influence which the Turanian conquerors (Turks, Mongols, perhaps Saka) have had in raising the kephalic index. It seems to me that these invaders, in the historical period at least, usually crossed the Punjab to settle in the richer valley of the Ganges. It is true that there is no indication of them in Risley's figures for the north-west provinces, either; but then he has not included the up-country Mussulmans, Moguls, Rohillas, etc., in his observations, which seems to be an unfortunate omission.

Another thing which some may regret is the exclusion from the scheme of any observations on colour. Mr. Risley appears to have been unacquainted with Broca's colour-scale (which, though not applicable to the hair, is fairly good for the skin), and to have thought that no easily workable system of colour-observation existed.

The Punjabis are, as is generally known, strikingly superior in stature to the people of other parts of India. Of 80 Sikhs measured the range was from 165 to 190 centimetres (65 to 74·8 inches), and the average 171·6 (67·5 inches); and the lowest average for a single caste in the Punjab (165·8 for the Aroras) surpasses the highest for any caste in Bengal (165·6 for the Brahmans). In weight, however, they fall below the burly mountaineers of Sikkim; and even among the stunted hillmen of Chota Nagpur there is one tribe, the Munda, which excels them. The proportion of weight to height varies, therefore, very remarkably, from 372 in the Mundas to 290 in the Khatris of Oudh, a caste supposed to have a strong Aryan element, the average for the whole of Bengal being apparently 308. On the whole the tall slender frame seems to characterise the partly-Aryan Hindu to-day, as it did in the Vedic period, when his

supposed ancestors derided the stumpy, noseless, black aborigines; still, it may be that differences in elevation of habitat have exerted some influence.

Other and more unequivocal characteristics seem to be those derivable from the breadth of the nose, and somewhat less positively from the naso-malar index of Flower and Oldfield-Thomas, on both of which great stress is laid by Risley, whose figures seem to bear out his opinion.

The average nasal index of the Punjabis comes out at 70·2, just on the limit of the leptorhine and mesorhine class, but the Sikhs, Pathans, and Biluchs are actually leptorhine, and the index for the Goojurs, the very interesting pastoral caste of the Punjab, is as low as 66·9; unfortunately only 13 Goojurs could be examined. These figures are perfectly European.[1]

The nasal index does not, as might perhaps have been expected, increase with anything like regular progression as we advance eastwards from the Punjab; but it does increase, and that considerably. Something should perhaps be allowed for the fact that a partly arbitrary, partly fortuitous selection of castes cannot be relied on to give the absolute means of the whole population; and something, certainly, for the personal equation of the observers; for the exact measurement of the nose is one of the most difficult bits of anthropometry. But it seems strange that Risley's Bengalis yield a nasal index of 78·7, his Bēharis one of 80, and his men of the upper Ganges (Oudh, etc.) one of 80·9. Similarly, his Brahmans of Bengal give us 70·4, those of Bēhar 73·2, those of the upper Ganges 74·6. Can it be that there is actually a larger Aryan element in Bengal than in the country between there and the Sutlej? That seems very improbable. On the other hand, the Mongoloid brachykephalic people of the Darjiling Hills have a comparatively low mesorhine index—only 74·7 on an average, and the Lepchas of Sikkim are positively leptorhine, while the people of the Chittagong Hills have a mesorhine index of 82·7, not nearly so high as that of the more dolichous

[1] I found the nasal index 69·1 in 50 Englishmen, mostly of the upper class.

hillmen of Chota Nagpur (87·4). The low-caste men of Bengal proper have a less prominent nose root (which is a simpler way of saying a smaller naso-malar index, 111·7 to 113·8) than those of Bēhar; though more prominent than the people of the Chittagong Hills, of Sikkim, and the Himalaya, or than them of Chota Nagpur, which facts, on the other hand, would seem to imply a greater infusion of Aryan blood in Bēhar than in Bengal. The Chota Nagpur people again are not quite so flat faced as—have a rather more prominent nose root than—the Chittagongers and Sikkimites (110·4 to 107·1 and 108·6). And lastly, the Bengalis approximate somewhat to the last-mentioned tribes in kephalic index, the order in that respect being as follows:—

Oudh, etc.,	72·8[1]	Bengal,	76·9
Punjab,	74·2	Chittagong,	79·9
Chota Nagpur,	74·9	Sikkim, etc.,	80·7
Bēhar,	75·7		

and the high average of the Bengalis not being due to the low-caste men included; for it affects both high and low, the very highest averages being those of the Brahmans (78·7), the Kayasths (the writer caste, of doubtful origin but high in rank) (78·3), and the Chandals, a very low caste (78·1). The average of the Brahmans of Bēhar is only 74·9, and that of those of Oudh and the north-west 73·1 apparently.[2]

Here seems to be a kind of crux. The most likely ways out of it are the following, for some of the discrepancies seem beyond the possible range of the personal equation.[3]

[1] See further on.

[2] This is possibly an under-statement; see further on.

[3] Mr. Risley's figures, having so wide a basis, need no confirmation except with reference to the possible interference of the personal equation. I may mention, however, that five Bengali Brahmans and Kayasths, whom I measured long ago, yielded an average index of 80·3. The measurements in Barnard Davis's *Thesaurus*, a great many of which refer to the crania of natives of Dacca in Eastern Bengal, are also corroborative. Davis mentions a very important point, that Bengali skulls are often obliquely deformed, probably from some peculiarity in the way of nursing: this may perhaps a little exalt the index.

Either (1) the case must be put down to the credit of the transformists, and we must suppose that the influence of media in Bengal tends towards brachykephaly. The only influence of the kind I can suggest is that of the drinking water, which in that great alluvial plain may be deficient in calcareous salts.

Or (2) we must suppose that the views of Col. Dalton and his followers are so far correct that there was at some time or times anterior to history a considerable immigration into Eastern India from the north and east, consisting of course of brachykephals, who would permanently modify the cranial form of the inhabitants of Bengal proper, but who did not push forward into Orissa and Chota Nagpur in sufficient numbers or force to produce any permanent effect on the prevailing type there.

Not that this hypothesis (which, by the way, seems to be favoured by Mr. Risley)[1] is not free from very strong objections. Thus, it is generally supposed, and claimed by themselves, that the Bengali Brahmans have been strictly endogamous at least since their arrival in their present seats, whither indeed the immigration of some of their subsections is asserted to have been comparatively recent. We can but suppose that during the period when Buddhism had sway in Bengal its doctrine of equality prevailed to break up for the time the Brahman law of endogamy, which was re-established and made strict, and society reorganised, on the downfall of that faith. Thus only, too, can we account for a degree of family likeness in most of the Bengali castes, the despised Chandals, for example, coming out with dimensions not much unlike the lordly Brahmans.

We pass next to the second type, which may be shortly named Negroid. Mr. Risley would call it Dravidian, but this seems hardly justifiable as yet, until we know through more numerous and exact observations in other parts of the great region which is Dravidian in language, that this is

[1] Marco Polo gives an interesting account of the victory of Kublai Khan over the King of Mien (Burmah) and Bangala, about 1280 A.D.; but we cannot be confident that the Burmese were really the lords of Bengal at that period.

really the type generally distributed through it. As yet we can only say that certain low or outcast tribes in the south seem to resemble those of Chota Nagpur, whether Kolarian or Dravidian in language, which have been examined. Ceylon is the only portion of the great south where a Sanskritic tongue prevails, and though it is always dangerous to argue from language to race, it is tolerably safe to say that if at any one time and in any one province of the south an Aryan colony had settled in numbers anything like equal to those of the natives, they would have succeeded in importing and transmitting their language. It is on the whole, then, improbable that anywhere in the peninsula Aryan blood predominates; yet we are told that in many parts the prevailing type is handsome and neither Mongoloid nor Negroid or Australioid in aspect.[1] It is best to suspend our judgment as to the component elements of these southern people.

The nasal index is the most characteristic point in the measurements of this type. In the valley of the Ganges, except in Bengal proper, it seems to be the invariable rule that a high caste is distinguished from a low one by the smaller breadth of the nose. Of course this is true only *en masse*, and not always in the individual; even in the Sikhs, reputed (I believe) an extremely homogeneous tribe, this index varies from 55 to 82. Of the naso-malar index I am not disposed to think so highly, at least in its application to the living. The Chandals in Bengal, and the Doms in Chota Nagpur, come out by that test better than the Khatri of the Punjab. It must also be very difficult in practice. But, as Topinard says, it is well worthy of further trial.

The third type is the one usually recognised as Mongoloid or Turanian, and is prevalent in two of Risley's districts, the Darjiling Hills (*i.e.*, Sikkim and its neighbourhood), and the Chittagong Hills to the east of Bengal. The northern people are taller and considerably heavier

[1] The Todas of the Neilgherries, for example, who are tall, handsome in feature, and evidently not platyrhine, yet with but little except their physical aspect to dissociate them from other Dravidian-speaking tribes.

than the eastern, but in other respects no great differences appear in Risley's tables, except that his frontozygomatic index (the proportion borne by the minimum breadth of the forehead to the maximum of the zygomata taken as 100) is larger in Sikkim than in Chittagong as 76 to 74. Topinard, in his masterly review of Risley's work, objects to this index as of little value, because the minimum frontal breadth gives little or no indication of the breadth of the frontal lobe. The really valuable index, as he shows, is got by comparing the stephanic or maximum frontal breadth with the zygomatic. But in the living subject this is extremely difficult; and though I myself employ Topinard's index with profit, I am conscious that my data are often wide of the fact. On the other hand the fronto-minimal plan is easy though untrustworthy, and on the whole and in the main, or in a certain number of cases, does point in the desired direction. Thus the index so gotten singles out the Brahmans from the other Bengalis (82·4, the next in order being the respectable caste of Kayasths with 79·3), and thus brings them into line with the nine Punjabi castes or tribes, who with the exception of the Kachis in the N.W. Province are the only ones which rise above 80. Both sections of Mongoloids have faces longer as well as broader than Hindus, whence the vertico-kephalic index, or proportion of the maximum breadth to the total height of the head (chin to vertex), is not very significant.[1] And I cannot help entertaining serious doubts of the entire trustworthiness of the figures for Oudh and the N.W. Province, in which I seem to detect a marked personal equation,

	[1] Length of Head.	Breadth of Head.	Height of Head and Face.	Height of Forehead (vertex to inter-superciliary point).	Ear Height.
Punjab - - - - -	187	139	218	86	135
Sikkim - - - -	184	149	220	82	135
Chittagong - - -	182	146	218	83	135
Bengal - - - -	183	141	208	76	129
Chota Nagpur - -	185	139	208	79	127
Bēhar - - - - -	185·4	141	208	78	129
Oudh & N.W. Prov.	185	135	222	99	127

operating in the direction of an exaggeration of the vertical height and probably also a diminution of the breadth of the head. Thus, if we compare, in the adjoining provinces of the north-west and of Bēhar, castes identical in name, and, we may presume, nearly identical in blood, we get the following results:—

		Length	Breadth	Vert. Height	Do., Forehead	Ear Height
Brahman -	Bēhar - - -	187·8	140·8	214	83·7	132
	Oudh and N.W. -	187·5	137·2	223	103·3	130·4
Babhan -	Bēhar - - -	187·8	144·4	214	80·7	132
	Oudh and N.W. -	187·2	137·4	222	99	129·5
Goalà -	Bēhar - - -	185·4	141·4	209	78	130
	Oudh and N.W. -	185·2	135·2	217	96·7	127
Chamar -	Bēhar - - -	184·4	140·3	205	76	129
	Oudh and N.W. -	185·1	134·9	221	99	127
Musahar -	Bēhar - - -	183	138·6	202	73	127
	Oudh and N.W. -	181·7	134·8	214	92	130
Mean of 5	Bēhar - - -	185·7	141·1	209	78·3	130
	Oudh and N.W. -	185·3	135·9	219	98	128·8

Here the correspondence of the figures in the first and fifth columns is so close that the wide discrepancies in the second, third, and fourth columns become almost incredible; and as they invariably run in the same directions of less breadth and greater height, for the castes of Oudh, etc., one is almost irresistibly led to attribute them, at least in part, to the personal equation of the observer. It is not difficult to conceive the habitual error in the *horizontation* of the head, which has led to the errors in the third and fourth columns: in the second the fault, if fault there was, lay probably in measuring at the spots where the maximum breadth might be expected to be, instead of groping about to find the maximum.[1] I see no internal evidence to lead one to distrust the other measurements, but of course all the indices for Oudh and the N.W. Province, into which the vertical height enters as an element, must have come out too small.

The very extensive distribution of some of the castes supplies an argument against the ethnological explanation

[1] Or it may be that the Oudh operator put a little more pressure on the callipers than did his fellow-workers. The frontal and bizygomatic measurements for Oudh, etc., are smaller than those for Bēhar, but not beyond the limits of likelihood.

of caste. Thus the Doms are found by name not only in the Gangetic provinces, in Chota Nagpur, and (I believe) in Orissa, but also in the far south, about Madras. And their occupations in these different localities seem sufficiently alike to lead one to identify them as, at least, the same caste everywhere. Are we to suppose that their wide spread is due to migration? In the case of the Brahmans and of the military castes or races, one would have no difficulty in believing this; but does it not seem difficult in the case of Helots like the Doms?

The study of the nasal index is enough by itself to make me believe in the racial origin of the system; but it seems quite possible that castes were formed subsequently by the comradeship of people of like occupation. In recent times and in our own day, castes would have been and are being formed out of aboriginal tribes low in the scale of civilisation or social status; and of this Mr. Risley gives many interesting and sometimes amusing instances. The chiefs of such a tribe almost invariably endeavour to hang themselves on to some branch of the Rajputs; while the proletariat constitute a new caste, for which an Aryan ancestry is sometimes claimed. Outcasts from higher castes are sometimes received into lower ones; and this may be one of the ways in which a circulation of blood has been set up, so as slightly to assimilate the various sections of the population.

The small average size of Hindu heads is remarkable. It comes out very distinctly in Barnard Davis's *Thesaurus*, as does their inferiority in this respect to those of the Mongoloid races to the north and east of their habitat. Even the Punjabi heads are not large, nor apparently those of the dolichous tribes north-west of the Indus, Dards, Kafirs, etc., of whom Leitner, Garson, and others have written. Admixture of Dravidian or Australioid types could only be expected to lower the cranial capacity; admixture of Mongoloid ones might possibly counterbalance this. Mr. Risley's facts seem to follow these rules. I have constructed a table of comparative sizes by multiplying in every case the maximum length by the maximum breadth,

and that by the ear height. A trifle less than two-fifths of the product would probably give the cranial capacity in cubic centimetres.[1]

Relative size of head in the several provinces :—[2]

Brahmans, Bengal	- -	3474	Pathans, Punjab - -	3626
Kayasths, „	- -	3394	Goojurs, „ - -	3602
Province, „	- -	3347	Province, „ - -	3521
Bagdis, „	- -	3277	Chuhras, „ - -	3393
Babhans, Bĕhar	- -	3653		
Brahmans, „	- -	3496	Cheros (Ch. Nagpur) - -	3544
Province, „	- -	3384	Province „ - -	3346
Musahars, „	- -	3218	Bhuiyas „ - -	3229
Brahmans, N.W. and Oudh		3347	Sikkim Province - -	3726
Chhetris, „		3342	Chittagong Province - -	3585
Babhans, „		3330		
Kayasths, „		3239		
Province, „		3197		
Kols, „		3083		

Here the brachykephals surpass the dolichos, the Aryans the aborigines, the upper surpass the lower castes. But the Brahmans, despite their claims to be considered a kind of intellectual aristocracy, do not seem to surpass the other high-caste men, unless it be in the dimensions of the forehead ; and the Kayasths or writers, almost all of whom live by their brains and their pens, do not stand very much above the average. These differences are not inconsiderable ; they exceed probably those which obtain between the superior and inferior classes in our own country ; but I do not think they yield any evidence in support of the inheritance of acquired characters.

[1] Thus : high-caste men (chiefly Bengali), 72 cubic inches capacity ; low-caste men, 71 ; Central India, 64·5 ; Lepchas (Sikkim), 73·8 ; Bodos, 75·5 ; Bodpas, 76·5 ; Mishmis, 77 ; Thai (Siamese), 77·6. (B. Davis.)

[2] The Chhetri (Rajputs), Babhans, Goojurs, have strong claims to be considered largely Aryan. The Kayasths, or writer caste, though now high in rank, are probably much less so. The Cheros were anciently a royal race. The Bagdis, Bhuiyas, Kols and Musahars (hunters) are distinctly aboriginal. The Chuhras are a helot caste.

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JOHN BEDDOE.

THE PRESENT POSITION OF FLORAL BIOLOGY.

DURING the last twelve months several important books and papers (1) dealing with the natural history of the flower have been published, and it may be well at this period to review our position, and also to endeavour to trace the general ideas that hold sway in this department of knowledge. The republication of C. K. Sprengel's *Entdeckte Geheimniss der Natur* (1793) enables any one easily to compare our present knowledge with his, and it is rather humiliating to find how very little we have advanced in the direction of deeper understanding of floral phenomena. We have indeed accumulated a stupendous mass of details, but except that for Sprengel's old-fashioned teleology we have substituted the modern one based upon evolution, invoking "adaptation" where he refers to "design of nature," we are really not much further forward. The chief advances made have been effected by Darwin and by H. Müller, who have given us the "Knight-Darwin-Müller hypothesis" and the "Theory of Flowers". No branch of biology has suffered more than that under consideration, from the pushing to extremes of the adaptation idea. The dangers of this course have often been pointed out, and in other branches not without good effect; but a perusal of almost any work upon floral biology will show that here at least, adaptation is made to explain almost everything; biological meanings are forced into every detail of floral structure, often without any evidence whatever. The fact that the subject is so admirably suited for popular treatment, is probably responsible for much of this.

Rather than confess that we do not know the meaning of a particular structure or phenomenon, we give a glib "explanation" by saying that it is an adaptation to something. So far has this gone, that even among professional botanists there is an idea, perhaps unconscious, that the subject is exhausted and affords no room for good research,

whereas, in reality, no branch of vegetable physiology offers more, or more interesting, problems for solution. One factor that has deterred many from work of this kind has doubtless been its laborious nature; few persons are willing to undertake experiments or observations lasting over several years, preferring to do something which may be speedily finished and put into print.

With this introduction, we may go on to consider the chief departments of the subject. Of the various treatises above-mentioned that deal with the whole subject, that of MacLeod is much the best for treatment of principles, while Loew's works give all possible details. Müller's book, though a classic, is now very seriously out of date, and is no longer suited for general readers or for students. Much of what follows is based upon MacLeod's work; it is unfortunate that the book itself, being in Dutch, is available only to a limited circle of readers (it has a French abstract). Starting with the more general ideas that underlie the whole subject, we shall deal first with

CROSS-FERTILISATION AND THE KNIGHT-DARWIN HYPOTHESIS.

As stated by Darwin, this is "that no organic being fertilises itself for a perpetuity of generations, but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable". By his work on orchids, Darwin rehabilitated Sprengel, and gave a fresh impetus to the study of floral mechanisms. In the writings of the earlier workers (*e.g.*, in Hildebrand's *Geschlechtervertheilung*, 1867) may be clearly seen the tendency to explain all flowers as adapted in greater or less degree to cross-fertilisation, whilst self-fertilisation is usually regarded as being actually harmful. This mode of looking at the subject is natural enough when one takes as a starting-point such a group of plants as the orchids, with their extremely complex mechanisms for obtaining cross-fertilisation. Darwin's experiments upon cross and self-fertilisation show that the latter is not actually harmful in itself, but that, *in most cases, its offspring will*, ceteris

paribus, *be inferior to the offspring of cross-fertilisation, if placed in direct competition with them.*

At the same time that these researches were going on, many cases of regular self-fertilisation were being discovered, though in all of them, as the flowers were open and often contained honey, there was a possibility of a cross taking place. Müller pointed this out, and proposed to substitute for the Knight-Darwin hypothesis, another, based upon the above facts, *viz.*, "that cross-fertilisation results in offspring which vanquish the offspring of self-fertilisation in the struggle for existence". Cross-fertilisation was still regarded as the chief object, so to speak, of a flower, self-fertilisation as a *pis aller.*

Until recently no cases were known of plants which always fertilise themselves and had no chance of a cross. Burck has shown that *Myrmecodia* and other plants never bear open flowers, but always fertilise themselves in cleistogamic flowers.[1] Hence the Knight-Darwin hypothesis must be abandoned in favour of some more general hypothesis, which will include these cases. Naegeli, as a result of his experiments upon hybridisation, laid down the law that "the consequences of fertilisation reach their optimum when a certain mean difference in the origin of the sexual cells is attained". The experiments of Darwin and others support this law, though they are contradictory when applied to the Knight-Darwin hypothesis. The latter is seen to be a corollary, for certain cases, of Naegeli's law. We may, therefore, for the present, adopt this law as a basis for further study; MacLeod proposes further hypotheses wider than that of Müller, based upon this law, and upon Darwin's experiments.

That cross-fertilisation, in itself, is advantageous, is probable for most cases, but there are at least three factors in the resulting gain, which ought to be considered separately and not lumped together as is usually done. These are (*a*) fertility of the parent plant, (*b*) strength of constitution of the offspring, (*c*) fertility of the offspring. Darwin measured

[1] Cleistogamic flowers = closed self-fertilising flowers.

all these and showed that in most cases there was a gain in all three, but that it was usually very different in each; a large increase in one is generally accompanied by a small increase, or even a decrease, in one or both of the others. If we represent the value of a in crossed plants by a_x, in self-fertilised plants by a_s, we find, for example, in *Dianthus caryophyllus*—

$$\frac{a_x}{a_s} = \frac{100}{92}, \quad \frac{b_x}{b} = \frac{100}{86}, \quad \frac{c_x}{c_s} = \frac{100}{65},$$

whilst on the other hand, in *Nicotiana Tabacum* the ratios are $\frac{100}{150}$, $\frac{100}{178}$, $\frac{100}{97}$ (second generation in each case). So that, in considering the advantage (if any) of cross-fertilisation in any given case, each factor must be treated separately, remembering at the same time that a certain (numerical) gain in a may mean more to a certain plant than an equal gain in b or c, and so on. We must remember further that *the plant has to pay a certain price for its cross-fertilisation, viz.*, the material and energy represented by large corollas, honey, scent, extra pollen, big stigmas (in wind-fertilised flowers), etc. The price to be paid will vary considerably, even in the same species, being affected by the climate, the competition of other flowers, the number and nature of the insects in the locality (these varying from place to place, and from month to month of the year), and other factors. Again, cross-fertilisation means delayed fertilisation, and in the short summer of arctic or alpine climates where there is but little time for the ripening of seed, this may become an important point.

Similarly there will be variation in the gain to be derived from cross-fertilisation in any given case. The factors a, b, c, will enter into it in different degrees and sometimes one may be more advantageous than another. We know too that the fertility of a plant varies from one country to another, so that the gain derived will vary too. A further advantage probably lies, to many plants, in the increased variability resulting from cross-fertilisation. It is thus evident that in dealing with the natural history of a flower we have a very complex problem to solve, and must

make up for every plant in any locality a sort of balance-sheet of profit and loss before we can fully understand its floral mechanism. *Only when the gain derived from cross-fertilisation is greater than the price to be paid for it, will it prove advantageous and be retained by the plant.* It is important to remember, in this connection, that in all probability the flowering plants began with cross-fertilisation, handed down to them by their cryptogamic ancestors.

Upon this enlarged basis we can now understand better the great variation in floral mechanism exhibited by the same species in different parts of its distribution area, and it becomes of interest to study it in every possible locality. We might suppose such a case as this: Two plants A and B of different species are removed from a district rich in insects, where they were both crossed, to a district poor in insects. If crossing had proved of great advantage to A, but of small to B, then we may expect to find that A will acquire increased attractiveness, paying the extra price demanded for cross-fertilisation, whilst B will tend towards self-fertilisation, the extra price being too great to make crossing any longer an advantage, whilst by reduction of corolla, etc., a distinct saving is made, which may be devoted to extra storage in the seeds, vegetative reproduction, or other purposes. It will be asked, why do not such flowers as B become cleistogamic, and so make the utmost possible saving? On the older view, it was said that they remained open upon the chance of obtaining a cross, but this no longer suffices as an explanation. It seems possible that the opening of the flower, the honey-secretion, etc., may be useless survivals from insect-fertilised ancestors, and that the necessary circumstances (see below) have not arisen which would cause their complete reduction.

The world-wide distribution of many self-fertilised plants has always been difficult to explain, but Wallace has proposed a solution which harmonises with Naegeli's law, suggesting that small changes in conditions of life, such as are obtained by moving into new localities, may yield the same advantages as crossing of different stocks in increased fertility and flexibility of constitution. The wide distribu-

tion of highly specialised cross-fertilising flowers is largely prevented by the lack, in new districts, of insects suited to them. Cross-fertilised wind flowers, on the other hand, are often widely scattered, and so are the plants of many orders such as Compositæ, whose mechanism is very effective for cross-fertilisation, but not specialised for too narrow a group of insects. To say, as many writers from Axell onwards have said, that self-fertilised flowers are the highest type, and best fitted to survive in the struggle for existence, is to go as much too far towards one extreme as the earlier workers went towards the other.

MacLeod makes a tentative grouping of flowering plants into two classes—"capitalists" and "proletarians". The first group is composed of plants which have large reserve stores and can afford to pay for cross-fertilisation by producing large and attractive flowers, etc., while still retaining enough material to supply the seeds. Such are our trees and shrubs, most perennial herbs and many annuals, whilst the second group is composed chiefly of annual and ephemeral plants whose supply of material is all or nearly all used for seeds, and which therefore, being unable to pay for crossing, are self-fertilised. Such are *Alchemilla arvensis, Scleranthus annuus, Radiola linoides.* The idea is suggestive and brings out a point to which too little attention has been paid (though Darwin saw its importance), *viz.*, the great advantage to a plant of having its seeds provided with a large supply of reserves, thus enabling them to germinate rapidly and get the start in the struggle for light and air. The few data (2) we possess upon the advantages of heavy seeds as against light ones go to show that the advantages of the former are at least as great as the advantages of crossing, so that it might well pay a plant to fertilise itself and produce heavier seeds rather than try to obtain cross-fertilisation. Of course there is here also a balance to be struck between conflicting elements. The heavier the seed the less its chances of distribution. Again, in the case of annuals, where the seeds are liable to germinate in autumn and be killed by the frost, it becomes advantageous to produce great numbers of seeds

rather than heavy seeds, and so on. Plants with a good distribution mechanism can, on Wallace's hypothesis, better afford to do without cross-fertilisation. When both occur together, we get cases like the Compositæ, of widely scattered dominant and aggressive families.

Another great factor in a plant's life history is vegetative reproduction. Similar considerations apply to this as to the two great factors already considered. In itself the most certain and economical mode of propagation, it has to be paid for by the loss of the advantages of fertilisation and rapid distribution, and so we do not find many plants possessing only this means of multiplication.

FORMS OF FLOWERS.

Since the publication of Darwin's work but little has been done until quite recently (3). Vöchting has experimented upon the effects of light upon the production of cleistogamic flowers. In species which in nature produce both these and ordinary flowers, a reduction of light to a sufficient extent causes the production of cleistogamic flowers only, and in all cases reduction of light tends to hinder, or even prevent altogether, the full development of the flower. Other workers have shown that probably low temperature, poor soil, etc., may also act as factors in the production of cleistogamy. It would thus appear that cleistogamy is a phenomenon largely dependent upon external causes, although in many plants it is sufficiently fixed and well-marked to be termed an adaptation.

Vöchting's experiments also bring out the interesting point that reduced light causes increased vegetative multiplication, which is in accord with the actual facts observed in northern regions, etc.

Gynodiœcism, with its allied phenomena, has also received considerable attention. A species is said to be gynodiœcious when there occur two separate forms, one bearing female the other hermaphrodite flowers. If both kinds of flowers appear on one plant we speak of gynomonœcism. If one form bear male, one hermaphrodite flowers, the species is androdiœcious. The great number of

observations made upon the floras of different regions enable comparisons to be instituted, and it is found that a species may be gynodiœcious in one country and not in another; and that, further, in any one place, the proportion of hermaphrodite to female flowers may vary enormously, both from one year to the next, and within the limits of a single year. A plant which is at one time hermaphrodite may even change to female in a short time, or *vice versâ*. The phenomenon may under certain circumstances (*e.g.*, in early spring or in late autumn) appear in almost any plant. Again, nearly all species which are usually gynodiœcious exhibit gynomonœcism also, female flowers occurring in greater or less number upon the hermaphrodite plants. Gynomonœcism also is a phenomenon varying in degree and from place to place.

So variable and inconstant is gynodiœcism that it can scarcely be regarded as hereditary, though it usually appears in certain Labiatæ and one or two other plants. It has not been shown to be in any way advantageous to the species possessing it, but it has been taken for granted that it is an adaptation to cross-fertilisation. After what has been said above it would seem better to remove it altogether, at present, from the realm of adaptation, and study it more thoroughly on its own account.

Similar considerations apply to the phenomena of andromonœcism, etc. Here the male flowers are usually at the distal end of the inflorescence or on small lateral twigs, and their production almost certainly depends on lack of nourishment. [Somewhat similar phenomena are seen in monœcious plants, so that there may be a closer connection between these phenomena than is usually supposed to be the case.]

A consideration of all that has been said so far leads naturally enough to the conclusion that one of the great desiderata just now is a study of the details of the nutrition of plants, more especially what we may term the *local* nutrition. We want to know the causes which determine the amount of food that goes to the various parts—the vegetative organs (including such as are detached for

reproductive purposes), the various flowers upon an inflorescence, the different parts of the same flower, the fruit, etc. The amount of material available to the plant is not unlimited, but depends upon the light-energy available and the amount and efficiency of the chlorophyll-apparatus. If a large expenditure be made in one direction, the plant must retrench in another. A beginning has been made in this direction by Vöchting's work, already mentioned, and by that of Burkill and Tansley upon the variation in number of the parts of the flower. In flowers such as Ranunculaceæ and Rosaceæ with indefinite stamens and carpels, the number of these decreases on the whole in passing up a raceme or down a cyme, and from the beginning to the end of the flowering season.

Of the remaining phenomena which come under this heading, little need be said. We are no nearer, apparently, to an understanding of the riddle of heterostylism than when Darwin's book appeared. Burck has shown that the Cassias, whose flowers have the style bent either to right or left, are really self-fertilising, so that Müller's idea that we have here a parallel case to ordinary heterostylism, must be given up. Müller is also responsible for another fanciful "adaptation"—the "sun" and "shade" forms of *Euphrasia*, *Lysimachia*, etc. He endeavoured to prove that the plants growing in shady places had smaller flowers, better adapted to self-fertilisation, than those in sunny spots. Whilst on the whole this has some truth in it, it cannot be called an adaptation, for we find both forms almost equally common in either situation, along with every possible intermediate condition. It seems in fact to be more a case of individual variability than anything else.

THE COLOURS OF FLOWERS.

These are usually regarded as being adaptations to insect visitors. Amongst the highest groups of insects, such as the butterflies, bees, drone-flies (Syrphidæ), etc., there is no doubt a considerable sense of colour difference, as Müller and others have pointed out. But among the lower groups it is rather doubtful if such is the case. The more

that observations upon flowers and their insect visitors multiply, the less certain seems the evidence of a colour preference among the lower classes of insects. If this be so, it is evident, since the evolution of flowers has gone hand in hand with that of insects, that only in comparatively recent times can the latter have produced any marked effect upon the colours of flowers, and that too mainly in the higher (biological) groups of flowers. Many wind-fertilised flowers, *e.g.*, *Rumex*, *Corylus*, the male flowers of many conifers, the females of *Abies*, etc., exhibit brilliant colouring. So also do the reproductive organs of many cryptogams.

Again we usually find it stated that the lines and spots of different colours, found upon the corollas of so many flowers, are adaptations to insect visitors—honey-guides, to use Sprengel's term. That they have a value in this way is pretty certain, but variegation is not uncommon in leaves or fruits, and is even found in flowers at points where it can have no reference to the honey. Flowers are very common that possess honey but have no guides, and *vice versâ*. There are many other interesting facts connected with colour in flowers, *e.g.*, the change of colour with age shown by *Diervilla* (*Weigelia*), many Boraginaceæ, and other flowers. This has been stated to be an adaptation (to show clever insects which flowers are not worth visiting), but is far more probably a simple chemical phenomenon, comparable to the colour changes seen in leaves and elsewhere. Another curious point is the three different colours (red, white, and blue), shown by the flowers of the common milkwort (*Polygala*). This seems to have some connection with the constitution of the soil, and also with the local nutrition, as all three sometimes occur on one plant. Sufficient, perhaps, has been said to show that we are far indeed from understanding the phenomena of colour in flowers, and that only secondarily and in comparatively few flowers can they be regarded as adaptations to insects. There is much to indicate that colour phenomena are largely bound up with sex and reproduction; the chemical changes occurring in the preparation of the reproductive and

sexual cells may perhaps often cause colour phenomena as mere "bye-products". The phenomena of chemotaxis, movement in response to chemical stimulus, seem to indicate a chemical difference between male and female cells, and we need not therefore be surprised at a difference in colour between male and female flowers such as is often seen.

DICHOGAMY.

When stigma and pollen do not ripen simultaneously the flower is said to be dichogamous; when they do, it is homogamous. If the stigma be first ripe, the flower is protogynous; if the pollen, protandrous. The multiplication of observations in various localities goes to show that this phenomenon also is very variable. In some regions a plant may be highly protandrous, in others only slightly so, or it may even be homogamous or protogynous. It has also been observed to vary with the season of the year.

FLOWERS AND THEIR INSECT VISITORS.

In this department of the subject an enormous mass of material has been collected. The literature relating to the European flora has all been collected and published in abstract by Loew. The results support the Müllerian theory of flowers very well, and are of interest in showing the great differences in the floral phenomena between different localities, even in plants of the same species. This point we have already considered.

A commencement (4) has been made upon the British flora, with the result of showing that short-tongued flies perform to a large extent the work done by the smaller bees, etc., upon the continent. MacLeod has worked out the flora of Flanders in a most thorough way, and a good deal of other similar work has been published.

MacLeod has also investigated the annual development of the chief groups both of flowers and insects in Flanders. For each natural order of flowers the number of species in flower in each month was noted; the resulting curve exhibits in each case a gradual rise to a maximum and a

steady fall. The larger insect groups (coleoptera, long-tongued flies, etc.) were treated in a similar way, the number of species of each that visited flowers being noted, but the curves obtained were never so regular, and in some cases went up and down in a most erratic way. It was shown however that the groups of insects have their chief maxima at different times, *e.g.*, the Lepidoptera in July, the short-tongued bees early in the season, and so on. Other papers of the same nature have also been published lately.

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The literature of this subject is very fully indexed in Müller's *Fert. of Flrs.* (to 1882), in MacLeod's paper in *Botan. Jaarb.*, 1890 (to 1889), and in Loew's book (to 1894), and other books and papers above mentioned.

JOHN C. WILLIS.

PRESENT KNOWLEDGE OF THE MECHANICAL TESTING OF IRON AND STEEL.

ENGINEERS have long recognised the need of testing the materials employed in the machinery and structures they construct, but it is undoubtedly owing to the rapid development of new materials that the question of testing has come so much to the front in recent years. Since 1852, when the first Werder machine was constructed for the Railway Commission of Bavaria, the advance has been extremely rapid, and the accuracy of the machines now at the disposal of the engineer has led to more thorough and complete methods of testing; errors have been eliminated, until at the present time the scientific testing of the strength and other properties of materials is carried out in as exact and careful a manner as in any other physical determinations.

This has largely been brought about by the establishment of state testing stations in many places on the Continent, all of them equipped with the most perfect appliances and under the direction of most able investigators; the publications issued from time to time from them are perfect store-houses of information on this subject.

The establishment of such testing stations in this country is perhaps neither desirable nor necessary, but it is decidedly a pity that the great technical societies have not, with one exception, used their funds more generously in providing the money necessary for research on many of the yet unsolved problems of the strength of materials. There are now several colleges in Great Britain possessing admirably organised laboratories under the charge of men capable of doing good work in this direction, but hitherto their work has been cramped for lack of funds to carry out what must, from the nature of the case, be often most expensive experiments. It can hardly be expected that the colleges can provide from their generally very scanty funds the grants needed for such purposes; it is often as much as

they can do to keep up the regular teaching work of the laboratories.

The field covered by the mechanical testing of iron and steel is too wide to be fully dealt with in this article; it is proposed therefore to confine attention to some of the more important points, to briefly state the extent of our present knowledge on them and to indicate the lines upon which it is desirable that further advance should be made.

Forms of Test Bars. It is certainly a matter of great regret that some system of uniform proportions for specimens for testing has not been adopted. As the matter now stands it is often extremely difficult to compare the results obtained by different experimenters, especially in the case of tensile tests when dealing with what is usually termed the ultimate extension.

In hard materials this difficulty does not arise, because after the limit of elasticity has been passed there does not occur to any extent that remarkable change in the molecular condition of the bar which is so pronounced in the ductile irons and steels, the bar in fact does not pass into the semi-plastic condition.

With ductile materials, however, as soon as the Yield Point has been reached the material passes into the semi-plastic condition, and just before rupture occurs we are confronted with a condition which most materially alters the ultimate extension. Up to the point in the test at which this occurs, it is when the maximum load which can be carried is reached, the extension has been fairly uniform all along the bar; if the bar broke at this stage the percentage extensions of all bars would no doubt be very strictly comparable, the extension being then proportional only to the length of the original bar for material of the same quality. But as soon as the maximum load is reached the extension becomes local, and the specimen draws down at the point at which fracture finally occurs; occasionally it draws down also at some other point to a certain extent. This local extension being confined to a very short length increases the ultimate extension of a short test piece proportionately much more than a long one.

M. Barba enunciated some years ago certain laws in regard to this point. He stated that in cylindrical test pieces of similar form the percentages of extension at fracture are the same for similar material, and that in the case of bars of the same length but varying in their cross-sectional area, or of bars of the same cross-sectional area but varying in their length, the percentages of extension vary as the ratio of length to diameter, and decrease as this ratio increases.

These laws have been very fully investigated by Mr. L. H. Appleby in a paper contributed to the *Proceedings of the Institution of Civil Engineers* (vol. cxviii., p. 395), embodying the results of a long series of experiments carried out at University College, London. The materials experimented with were two qualities of Bessemer steel, a hard and a soft quality, both specially prepared for the experiments. Briefly, the results of the experiments may be said to conclusively verify Barba's laws. They also showed very distinctly that the local contraction before fracture was enormously affected by local hardness or impurities, a point before observed by Bauschinger and others.

There can be little doubt that much better results for the sake of comparison, and even for judging the quality of a given material, would be obtained, if the final extension on some fixed short length which includes the fracture were discarded, and only the rest of the ultimate extension given when recording any experiment. Probably the most convenient standard for cylindrical bars would be obtained by making the ratio of length to diameter 10. The whole question was very fully thrashed out as regards English engineers, in the discussion on the late Mr. Hackney's paper on "Forms of Test Pieces" (*Inst. Civil Engrs. Proceedings*, vol. lxxvi., p. 70); it was then pointed out that there were great practical difficulties in the way of the adoption of a fixed ratio of length to diameter, owing to the fact that in testing gun steel, tire steel, etc., the specimens must of necessity be made very short, hence with a fixed ratio of length to diameter such as 10, the cross-sectional dimensions of these specimens would be unsuitably small.

Where, however, tests are made of bars of iron and steel, and where there is no question of this difficulty, it would be a great convenience in making comparisons if a standard value for the above ratio were adhered to. The German system has been to make the cylindrical test bars always 10 diameters long; in recording the extensions they also give the extension on only one half the original length, taking the point of fracture as the central point of this length. For flat specimens the ratio of width to length is 1 to 5, and the natural thickness of the plate or bar is adhered to up to about three-quarters of an inch, above this round specimens of standard dimensions are turned out of the material.

The Admiralty length for steel plate specimens is 8 inches, and according to the German rule this would be accompanied by a width of 1·6 inches, a reasonable and practical value.

There is no necessity to adhere slavishly to such rules, but it does seem absurd to put aside the obvious advantages to be obtained by working on these lines whenever it is possible.

Reduction of Area at Fracture. Many experimenters and engineers lay great stress on the value of this quantity as a guide in estimating the ductility of a material, and undoubtedly if taken along with the condition of the fractured surface it is of much value. But as was pointed out by Bauschinger, and shown very clearly by his museum of specimens, a comparatively minute flaw or hard spot at the point of fracture would frequently diminish the reduction of area to an extent apparently far in excess of the proportion of the flaw to the rest of the fracture surface. Similarly with flat specimens, imperfectly formed ends, though they influence but little the maximum load carried, or the ultimate extension, often seriously effect the reduction of area, sometimes making it apparently almost zero.

It cannot, therefore, be contended that this factor is such a reliable guide as the ultimate extension in judging the quality of a material. It is well, however, to dispel a statement often put forward against the value of the figures recorded

for the reduction of area in tests. The author has measured the reduction of area of thousands of specimens, and never, or only in the case of exceptionally badly welded wrought iron specimens, experienced any difficulty in making perfectly accurate measurements of the reduced dimensions. The reduction of area, in ninety-nine cases out of a hundred, can, with the help of vernier callipers, be measured quite as accurately as the ultimate extension ; the latter, in the case of irregular fractures owing to the difficulty of fitting the two halves together, cannot very often be determined with accuracy. The objection, therefore, to measuring and recording this factor owing to these supposed difficulties falls to the ground ; in the few cases where it is impossible to do it the material is usually too defective to pass muster.

As to whether it is a fair test of the ductility of the material is quite another matter, but it certainly does materially help one in coming to a conclusion.

It is often stated that the percentage of reduction of area at the point of fracture rarely exhibits any relation to the percentage of ultimate extension, and that for this reason it is unadvisable to use it. For a perfectly plastic material it can readily be shown that the percentage contraction of area is directly proportional to the percentage of elongation, expressed however not in terms of the original length but of the final length. Now no test piece is truly plastic in the latter part of the test except, perhaps, in the immediate neighbourhood of the place of fracture, and therefore it would be impossible for any direct relation to hold between these two quantities, when the extension refers to the whole bar. If, however, the percentage of elongation is obtained for a short length only, which includes the fracture, say for one inch of the original length at that point, and this can always be done if the whole bar has been marked off in inch lengths prior to the test, then it will be found that this quantity is usually in close agreement with the percentage reduction of area, because we are now dealing only with the portion of the bar which is very nearly truly plastic. In my own testing work I always have the bars marked off into inch lengths, and make these measurements which I find of great value.

Since this involves but very few extra measurements and very little time, in fact they can all be made and the results worked out while the next bar is being fitted into the machine, it is a pity they are not systematically recorded in all tests.

Work of Fracture. Professor Kennedy pointed out in a discussion at the Institute of Civil Engineers (*Proceedings*, vol. lxix., p. 30), that the amount of work spent in fracturing any bar, expressed in inch tons per cubic inch of the bar, was a most valuable indication as to the power of any given material to resist sudden shocks or blows. Whenever an autographic stress-strain diagram has been drawn during the fracture of a bar it is an easy matter to calculate the work of fracture, since it is proportional to the area enclosed between the stress-strain curve and the axis on which the strains are set off, and its numerical value is known directly we know this area in square inches and the scales of stresses and strains; it can also be similarly determined when the curve has been plotted afterwards from the observed strains recorded during a test. But as both these operations involve a good deal of labour and time, it has generally been assumed that the determination of this factor was beyond the scope of ordinary testing. Professor Kennedy, however, in this discussion gave a formula for obtaining the work done in fracture, involving only a very short arithmetical calculation; he pointed out that all the data required in his formula were known from the observations always made during an ordinary test. The formula is that the work done in inch tons $= \frac{wx \cdot (r+2)}{3}$, where w is the maximum load carried in tons per square inch, x is the final extension in inches and r the ratio of the Limit of Elasticity to the Maximum load. By Limit is here meant the more scientifically termed Yield Point or break-down point, which is always readily enough indicated during the test by the steelyard drop and therefore involves no delicate instrument for measurement. All the three factors of the formula are known directly the test is complete, and with the aid of a slide rule the simple calculation required is made in a few moments without causing

any delay in the work of testing. No other result obtained in a tensile test shows more clearly, for instance, the utter untrustworthiness of wrought iron plate when stressed across the grain if it is liable to shocks or sudden loads, or shows more distinctly the superiority of steel plate in this respect.

Limit of Elasticity. No term has given rise to more confusion in dealing with the strength of materials than this. One instance will suffice to show the kind of error produced by false notions as to its meaning; the well-known fact that by stressing a bar in tension beyond its Yield Point we raised its Limit of Elasticity in Tension was usually considered to represent the whole result of the action. We know, however, from the researches of Bauschinger that this is a very imperfect and misleading conclusion, a conclusion, too, which may have caused much of the difficulty in understanding some of the results obtained in the endurance tests of Wöhler.

Bauschinger's experiments, carried out with perhaps more exactitude than any previous experiments of this nature, lead us to conclude that there is for any given material a true natural Limit of Elasticity, understanding by that term the Limit of the Elastic Condition according to Hooke's law, *ut tensio sic vis.* But this natural Limit may be varied in all sorts of ways, by strains set up in the material during manufacture, by after working, or in a testing machine. Unless, therefore, we know the whole previous history of the bar we are testing we are quite unable to say whether the Limit obtained in our test is a natural one, or whether it is some artificial one produced by the treatment the bar has undergone during manufacture or afterwards. In one form this fact was appreciated by all those interested in the question of testing; it was well known, for example, that cold rolling greatly raised the Limit, two bars rolled from the same ingot would give very different values for this factor, the one with the smaller cross-sectional area and therefore during the last stages of manufacture rolled while partially cold would show a much higher apparent Limit. But it was not, at any rate generally, understood that such effects were only particular cases of a much

more general law. This fact at once disposes of much of the talk often indulged in during discussions on this subject as to the value of high elastic limits when they are determined simply from a static tensile test; and the importance of the question is at once realised when we consider that these so-called higher values are made the basis of a claim for higher working stresses, even for cases where the loads are alternating tension and compression.

It is to be hoped, therefore, that this question of the determination of the Natural Limits of Elasticity of different qualities of iron and steel will be undertaken as a research work by some competent observers having the requisite delicate measuring apparatus at their disposal. Such a research if carefully carried through would settle many important points of the utmost practical value, and would at the same time confirm experiments which, from their far-reaching consequences, and from the fact that the deductions from them depend on extremely small differences in measurements, themselves very minute, much need confirmation.

Carried out in a systematic manner, careful chemical analysis being made of the material tested, we should obtain data to make it possible to deduce the relations existing between the values of the natural limits, and the proportions of the various constituents of other elements present in iron or steel, and probably be able to ascertain definitely the best proportions for a material required for some given purpose. It would also enable us to settle in a much more rational manner the working stresses which could be allowed in any given case; this, a matter of the utmost consequence in the case of parts of machines or structures subjected to alternations of stresses, is equally of importance where there are only variation of stresses or even only long-continued statical loads, for we are still without information as to the changes which may go on in the value of the Limit under these conditions.

That a steady change in the opinion of engineers in regard to this question of working stress has been in progress for the past few years is undoubted, but at present we are still groping more or less in the dark, because though

there is a mass of rapidly accumulating data, it at present seems largely to be only capable of being used in a more or less empirical fashion and still to be in need of some rational explanation. The researches of the Committee of the Institution of Mechanical Engineers on "Alloys" has no doubt done something to shift the question into a clearer light, but the field open for research is still very wide.

Repetition of Stress. The questions of the value of the natural limit and of safe working stresses are intimately bound up with that of the effect of repeated loads on the endurance of materials. Many engineers, taught by experience, often costly, had realised the importance of this question before Wöhler's classical experiments drew the attention of every one to this vital point. It is, therefore, to further research in this field, combined with the experimental work that I have just alluded to, that we must look for a truer insight into many obscure questions, and for the explanation of many apparently inexplicable failures and break-downs. It is, however, a most costly line of work, and one which from its character can only be adequately carried out, when funds are forthcoming either from the State or from the Technical Institutions interested in the question. The machinery needed is purely special, and must be continually at work, and it involves the use of a not inconsiderable amount of power constantly running, in addition to the time and attention of observers who must be largely free from other duties.

There is one difficulty in connection with this subject which is perhaps not sufficiently attended to in experiments on the effect of repeated loads, namely, in the determination of the actual range of stress in any given case. This can in most cases only be determined by certain calculations, which generally ignore, or consider as non-existent, effects due to vibrations set up by the instantaneous application and removal of the loads, or to inertia of moving parts of the machine. These may no doubt with carefully designed appliances be practically eliminated, but the first of them at any rate may possibly quite unexpectedly creep in, and in an experiment of this nature a very small error may

produce very serious results, perhaps deciding into which of two important groups of results a particular experiment is to be placed.

When it is remembered, that apart from inertia effects, the instantaneous stress produced by a sudden load, if it does not exceed the limit, may be twice as great as the static stress produced by the same load when applied gradually, it will be conceded how important the point is in such calculations; the real stress due to the load may be well within the limit of natural elasticity, and yet the instantaneous stress set up may exceed it, and thereby when often enough repeated seriously change the molecular condition of the bar.

This point is dwelt upon, because it seems to be too often forgotten when discussing the effects of repeated loads, and the results they produce in parts of machines or structures, and also in estimating the so-called factor of safety.

The lines therefore, to sum up, upon which further scientific testing should proceed are two: (*a*) The determination of the real natural Limits of Elasticity of different qualities of iron and steel, and the influence upon it of the various chemical constituents, and the way in which it is altered by various methods of treatment, that is, the question of artificially raised or lowered limits. In this connection it may be interesting to remember that the Modulus of Direct Elasticity is the one function which seems most constant, no matter how the quality of the iron or steel varies, still it does seem to change as the carbon percentage is increased, being gradually diminished in value as that percentage rises. May it not, therefore, turn out that there is a direct connection between the values of the natural Limit of Elasticity and the Modulus of Elasticity?

(*b*) The study of the effect of repeated loads, continued on the lines of Wöhler, Spangenberg, Bauschinger, and others, and in particular the solution of the question as to what is the molecular change induced by repeated loading beyond the range of the Limit of Elasticity, and why it is that the broken parts of the bar when tested at once in an ordinary testing machine show apparently no molecular

change comparing them with material which has never been stressed at all. Until this obscure point is cleared up we shall never be able to understand thoroughly this question.

Commercial Testing. In concluding this brief notice of the present condition of testing iron and steel, it may be worth while to state what it is desirable that the commercial testing, carried out only to determine whether the quality of a given parcel of material is up to the standard, should aim at. There is no doubt that simultaneously with the tensile tests what may for convenience be termed the workshop tests should always be rigorously insisted on, namely, for tires, axles, rails, etc., the drop tests; and for plates, bars, etc., the bending tests. These tests do at times detect faults which would not so readily be brought to light by the simple tensile tests of samples of the material only, but they should always be accompanied by the latter test. If they are properly carried out and an accurate record of the results obtained is kept they must throw much light on many important questions, but if, as is so often the case, they are done in a careless fashion with no precautions to eliminate possible errors, they will probably simply produce results so discordant as to be useless, and puzzling and unsatisfactory to both producer and user.

I have already dealt to some extent with the question of the form of test bars. Personally I would advocate for all flat specimens a uniform length of 8 inches in the parallel part, and a width of 1·6 inches for all thicknesses up to 1 inch; beyond this it is better to have turned specimens. The ends should if possible be at least 2 inches wide and long enough to secure a good grip of the wedges, though for moderate thicknesses perfectly plane parallel strips will give quite trustworthy results. For all ordinary turned specimens a ratio of length to diameter of 10, but in the case of tires, axles, gun steel, etc., a ratio of $2\frac{1}{2}$ to 1 will probably be found best, the ends being formed either with a collar to pull from or screwed. Though for scientific work it is advisable to pull through a spherical bearing, it is not of much consequence for ordinary work; the ends must be exceptionally badly formed and held to

produce any appreciable effect on the results recorded in such testing.

Should the metric system become compulsory it would certainly be advisable to try and obtain an international agreement as to the leading dimensions for ordinary test bars.

Next as to the observations which should be carried out and recorded. Every test record sheet should have a column in which could be briefly recorded the past treatment of the specimen, such as whether it had been turned down or forged down to size, whether sheared off, annealed, and so forth, for without some such brief history it is almost impossible to properly interpret the test records.

The original dimensions should be given, the Yield Point as determined by the steelyard drop, and the maximum load carried with the ratio of these two, both of course expressed in pounds or tons on the square inch; also the whole extension expressed as a percentage of the whole original length, and the extension on the two inches or one inch which includes the point of fracture (according to the original length of the bar) also expressed as a percentage; lastly, the reduction of area per cent., meaning by this the ratio of the lost area to the original. These observations are perfectly easily recorded in every test, and with no more time spent, as repeated experience has convinced me, than when some of them are omitted. Finally, I would have the work spent in fracturing the bar, calculated as above, per cubic inch, entered on the record sheet, and a brief note as to the condition of the fractured surfaces.

With figures such as these before him from the test house, with similarly carefully recorded results of the impact and bending tests, no engineer would have the least difficulty in coming to a decision as to the value of the material the maker was supplying to him, nor would he ever have the slightest future cause for anxiety in regard to any material which had successfully passed the ordeal. Checked by the chemical analysis the maker too would know the lines on which he was working, and how and where he could improve the quality of the material he was putting on

the market, or how he could produce something which would come up to the particular standard fixed for some special work.

It is most desirable too that there should be a few machines in every country capable of dealing with full-sized members of structures, such as built up struts, tiebars, etc., this has been too much neglected in Great Britain; in regard to full-sized struts we are in need of much more detailed experiments before anything can be done to put the empirical formulæ now in use on a more satisfactory footing. In fact, as a rule, compressive tests to be of real value must be made on the column or strut of the full size, or at any rate as near to it as possible, since it is almost impossible to deduce very satisfactory conclusions from the results of the tests of mere specimens.

Space does not permit me to deal with the equally important question of temperature effects on the resistance of iron and steel to stress, especially the effect of long-continued alternations of temperature and stress. Much good work has been done by Mr. Andrews and others, but much still needs to be done if we are to determine accurately the Physical Laws governing these facts.

T. Hudson Beare.

LOUIS PASTEUR.

LOUIS PASTEUR, one of the scientific leaders of the century, has passed from amongst us. His personal work is finished, and though the influence which this work will have on the progress of science during the next century cannot at the present time be calculated or foretold, even approximately, we already know how far his investigations have extended our knowledge of Molecular Physics, altered our ideas concerning certain biological problems, and revolutionised our conceptions of the Ætiology and Pathology of the specific infective diseases. It may be held by some, as Pasteur himself held, that the only record of his work that need be kept is that contained in the *Comptes Rendus* of the Academy of Sciences of Paris, but we are glad to say that a sketch of the life of the man who carried on the great work now inseparably associated with his name has been given to the world by his son-in-law, M. Valery Radot, under the title *M. Pasteur, Histoire d'un savant par un ignorant.*[1] For many of the facts of Pasteur's life I have drawn freely on this sketch, but the source from which most of the facts relating to his work are taken is that to which he himself would refer his followers—the *Comptes Rendus.*

Like so many of the greatest sons of France, Louis Pasteur was born, not in one of her great cities, but in Dôle in the Jura, the ancient capital of *Franche Comptes*, and originally the seat of the University College now at Besançon in the Doubs. His father,—a quiet, contemplative, and studious tanner was a retired soldier who for his valour had been decorated on the field of battle,—married an enthusiastic and ambitious woman, ambitious not for herself or her husband, but for the future of the son who was born to them on the 27th December, 1822. Father and mother alike determined to make of their son an "educated man"; what success they

[1] Translated from the French by Lady Claud Hamilton: *Louis Pasteur, His Life and Work.*

attained we are now in the best position to form an opinion. Three years after the birth of this son the Pasteur family removed to Arbois, in the valley of the Cuisance, so noted for its wine, where the lad received his early education in the Communal College, the father devoting his evenings to the superintendence of his son's lessons. Like most clever lads he was not noted for his studiousness at this period, fishing and portrait sketching occupying some of the time and energy that might perhaps have been devoted (with less advantage to the boy probably) to school work. Some of the sketches made at this time, when he was only thirteen, were so clever that it was afterwards said of him by an old lady friend: "What a pity that he should have buried himself in chemistry. He has missed his vocation, for he might by this time have made his reputation as a painter." However, Pasteur had a talent for work which could not long lie dormant, and he very soon set himself seriously to master all that could be taught him by his teachers. In order to complete his studies in Philosophy, Pasteur went from Arbois, where his interest in chemical study had been kindled by M. Darlay, to the College of Besançon ; here, at the end of his scholar's year, he received the degree of *bachelier-es-lettres*, and was appointed tutor in the college ; he was thus able to continue his studies for the entrance examination at the École Normale. In 1842 he passed fourteenth, but determined to devote another year to study for the same examination, with the result that in October, 1843, he was placed fourth on the list of successful candidates. He at once commenced to study chemistry at the École Normale under M. Balard, and at the Sorbonne under M. Dumas, from the former of whom he learned to be enthusiastic and voracious for facts, from the latter a love for precision and exact experiment.

Here, with leisure to read and every encouragement to carry on independent investigation, Pasteur now laid the foundation of his future work, and, under the encouraging direction of M. Delafosse, devoted himself to the study of Crystallography, especially in relation to the arrangement of the molecules in these crystals. As Tyndall

suggests, probably bearing in mind the properties of rock crystal, and the difference of the facets in the right- and left-handed crystals, he saw reason in Mitscherlich's note to the Academy of Sciences—that tartrates of soda and ammonia caused the plane of polarised light to rotate to the right, whilst the paratartrate, which in chemical composition is absolutely the same, causes no rotation—to make a careful search for facets like those that had already been found to exist in rock crystal. Such facets he was able to find in the tartrates, and this being done he assumed that the want of power of rotating light which existed in the paratartrate must be due to the combination of the opposite rotating powers of right- and left-handed crystals. He anticipated, indeed, that the tartrate was without a plane of symmetry, whilst the paratartrate was symmetrical, and that the crystals of the paratartrate would be all characterised by the absence of dissymmetry; on coming to examine the crystals carefully he found, however, that certain of them were dissymmetrical in one way, others in an opposite. "Some of these crystals when placed before a mirror produced the image of the others, and one of the two kinds of crystals corresponded rigorously in form to the tartrate prepared by means of the tartaric acid of the grape," and he argued that one of these forms of crystals must therefore be the one found in the right-handed tartrate, whilst the other must be something different, and he undertook the laborious task of separating the different kinds of tartrate crystals, placing them in two groups: those like the ordinary right-handed tartrate, and those which he found rotated light to the left. These experiments were carried out during his final year of study in the École Normale where he took his Doctorate in 1847, during the time that he was Professor of Physical Science in the Lycée of Dijon, and whilst he was acting as Assistant Professor of Chemistry in the University of Strasburg. (It was in Strasburg that Pasteur met his future wife—Mdlle. Marie Laurent.)

As a result of these physical and chemical observations Pasteur conceived the idea that there was an essential differ-

ence as regards symmetry between the products of inorganic nature and organic products, the one set having a superposable image, therefore being not dissymmetrical, the others having an image not superposable, and being therefore atomically dissymmetrical, "this dissymmetry expressing itself externally in the power of turning the plane of polarisation".

In 1854 Pasteur was nominated Dean of the Faculty of Sciences at Lille, and a new series of investigations was commenced.

It is curious that such a purely physical question as that above outlined should be the real foundation of Pasteur's contributions to the solution of some of the great biological problems that at this time were occupying the minds of scientific men. It was already known that impure tartrate of lime contaminated with organic matter and kept in water would undergo a process of fermentation during which various products were yielded. In order to examine this process Pasteur took about one hundred parts of the pure crystalline right-handed tartrate of ammonia, dissolved it, and added to the solution one part of albuminoid matter. He found that when this was placed in a warm chamber fermentation took place, and the clear solution became turbid, this turbidity being due to the appearance of a number of minute organisms which, following up the analogy of the presence of yeasts in alcoholic fermentation, he assumed must be the cause of this special form of fermentation. He then obtained similar fermentation in a solution of paratartrate of ammonia, when he found that the same organism still made its appearance. Following one of the changes that took place in the fermentation of the two salts with the aid of the polariscope he found that in the case of the paratartrate, rotation of light to the left could be demonstrated at an early age of the process, from which it was evident that the right-handed tartrate only was being attacked and broken down under the process of fermentation; this was allowed to go on until the deviation to the left had attained a maximum, "the fermentation was then suspended, there was no longer any of the right-handed acid in the liquid, which, when evaporated and mixed with its own volume of alcohol, immediately furnished a

beautiful crystallisation of left-handed tartrate of ammonia". If, however, he allowed the fermentation to continue beyond this point the whole of the tartrate was broken down. It was evident, then, that in this case these minute organisms have a power of selecting the special form of tartaric acid and that they have a preference for this, although, when this special form is used up, they fall back on the other and break it down in the same way as they have already used up the first.

In continuing these experiments, Pasteur made the first of that series of experiments by which he was able to corroborate Schwann's experiments designed to prove that no life could be generated from dead organic matter, but that, if certain organisms gained access, even to special forms of inorganic matter, they were capable of multiplying freely ; he was able to show that the spores of the Penicillium glaucum, or common mould, could grow and produce the fully formed mould, in a solution of paratartaric acid—to which nothing but phosphates of potash, of magnesia, and an ammoniacal salt of an organic acid had been added—first using up the right-handed tartrate and then attacking the left.

Liebig and Gay Lussac had both taught that fermentation and decomposition were due to the action of the oxygen of the air upon nitrogenous substances, and that this process was due to the initiation of certain molecular movements by this process of oxidation, these movements being gradually communicated from particle to particle of the fermentable substance, which was thus resolved into the products of fermentation. Others—Berzelius and Mitscherlich—explained fermentation as the result of the action of a nitrogenous ferment which exerted a catalytic action by its mere presence, adding nothing to the solution and taking nothing from it, but exerting its influence by what these philosophers termed the phenomena of contact. Pasteur was satisfied with neither explanation. Caignard-Latour and Dumas—Pasteur's teacher—both associated the growth of the yeast-cell in organic solutions with the process of fermentation that accompanied this growth.

Pasteur made his first experiments on fermentation with milk, in which he recognised, as the process of fermentation went on, the lactic ferment, which he described as little rods with slightly constricted centres, so small that their diameter was scarcely the twenty-five thousandth of an inch. This rod multiplied by a process of division, each one into two shorter rods which, in turn, assumed the shape and size of the original. To isolate this organism and to obtain a comparatively pure culture, Pasteur boiled a little yeast with from fifteen to twenty times its weight of water; he then carefully filtered the liquid, dissolved in it about 5 per cent. of sugar, and then added some chalk. This he seeded with a trace of the ordinary lactic fermentation deposit, with the result that the next day a lactic acid fermentation had set in, the liquid became turbid, the chalk disappeared, and a deposit of lactic ferment gradually took its place. He then repeated his previous experiment of adding this special ferment to small quantities of crystallisable salt of ammonia and phosphates of potash and magnesia. In this he found also, that although there was no organic matter present beyond that which he had introduced in the shape of his organisms, they still continued to multiply and to give rise to the typical fermentation, both yeast and the lactic ferment acting in the same way, each giving rise to its own special product. By these experiments both Liebig's and Berzelius's theories were at once shattered, as the carbon was supplied by the fermentable substance, whilst the process of fermentation could not be due to the action of oxygen on nitrogenous substances, as nitrogenous matter did not enter into the constituents of the solution, and still a vigorous fermentation had taken place. Pasteur concluded that fermentation was simply a phenomenon of nutrition—the nutrient substance being the sugar, the organism nourished being the ferment germs,—whether they were lactic acid organisms or yeast—the organisms contriving to obtain the materials necessary for the building up of their complex organisation from the sugar and the purely mineral elements.

Then followed the discovery of the butyric ferment,

which, unlike the lactic ferment, in so far as the organism by which it is brought about,—which Pasteur was enabled to separate,—exhibits the greatest activity in the absence of oxygen ; indeed, when a current of air was passed through the fermenting liquid, the organism—a vibrio—became motionless, fell to the bottom of the fluid, and the process of fermentation ceased. These two organisms were taken by Pasteur to represent two classes : (1) the *aérobies* or organisms which require a supply of free oxygen in order that they can live and carry on their fermentation function, and (2) the *anaérobies* which not only can live without air but flourish best in this condition.

In both cases the organism uses up for its own nutrition a comparatively small amount of the fermentable substance, but decomposes a much greater quantity of this material, breaking it down into what are known as the products of fermentation. He found, however, that yeast could bring about fermentation not only in the presence of oxygen, but also when it was absent, although the amount of decomposition was very different in the two cases. Where no air has access to the organism, a single part of yeast will break down from 100 to 150 parts of sugar ; whilst when oxygen is present it will only break down something like five or six parts, and Pasteur pointed out that in the one case the organism obtains a large part of the oxygen necessary for its growth directly from the air, and therefore does not require to break down so much sugar in order to obtain the elements that it requires for its nutrition ; whilst, when air is absent, the oxygen must be obtained from the sugar, as a result of which a very large amount of alcohol is produced during the rearrangement of the molecules rendered necessary by the abstraction of the oxygen.

Various forms of putrefactive processes were then studied by Pasteur, who was able to show that they depended upon organisms similar to those that he had already described in the processes of fermentation, that some of these were aerobic, some anaerobic, that they differed in character according to (1) the nature of the medium to be broken up, and (2) the nature of the organism that was engaged in

setting up the special fermentation. He pointed out, too, that one organism would carry on the process of decomposition to a certain stage, whilst another organism would carry the process a stage farther, so that ultimately the most complex bodies are broken down through the agency of these putrefactive and fermentative organisms into the simplest products, ammonia, carbonic acid and water, the anaerobic organisms doing one kind of work, the aerobic organisms another. He demonstrated that through their agency all organic matter, whether animal or vegetable, as soon as it loses its power of assimilating—in fact, as soon as it dies and becomes effete—is broken down into substances which may be assimilated by plants, living organisms in this way being enabled to live on the dead material that had been stored up by previous generations of these same living organisms.

Pasteur's investigations now, for a time, took a practical turn. The vinegar manufacture, upon which Orleans depends very largely for its industries, claimed his attention. The old process was tedious, uneconomical, and precarious. Pasteur, working at the subject, found that a pure culture of the *Mycoderma aceti* has the power of taking up oxygen and of transferring it to the alcohol of wine, through which process such alcohol is turned into vinegar. He therefore advised that a pure culture of the *Mycoderma aceti* should be sown in a mixture of wine and vinegar, and that this should be kept at a temperature of 20° or 25° C. At the end of a couple of days the *Mycoderma* covers the whole liquid, and in eight or ten days this is converted into vinegar, the process commencing immediately the plant begins to grow. If, however, the process is allowed to go on too far the vinegar is still further broken down, and ultimately every trace of acetic acid may disappear, this being the result of the action especially of certain anaerobic organisms which continue the process of breaking down, so that ultimately nothing but a foul or odourless and tasteless fluid may remain, each step appearing to be the result of one of these processes to which reference has already been made. These researches gave a new life to the Orleans' vinegar manu-

facture, and at the same time enabled Pasteur to demonstrate that his theory of fermentation, and not that of Liebig, was undoubtedly the correct one.

To-day it seems almost impossible that only so recently as a little over twenty years ago, the theory of spontaneous generation should have been thoroughly believed in, and that ingenious experiments were made and great ability displayed in defence of this theory as regards the development of living bacteria. Van Helmont's evolution of mice from a pot charged with corn and stuffed with a dirty shirt, and his statement that the smells which arise from the bottom of morasses produce frogs, slugs, leeches, grasses, and other things, are now looked upon as scarcely more extraordinary, although they date back a couple of hundred years.

Having studied the process of fermentation and noted the relation of effect to cause, and having also noticed that the commencement of fermentation and putrefaction processes were invariably associated with the process of seeding out, through the deposition of living organic particles in his nutrient media, he naturally—I say naturally because Pasteur was the observer and logician who was investigating the point under consideration—came to the conclusion that living organisms like all animal and vegetable cells were developed only by a process of reproduction or division of the parent organisms. So many difficulties did the problem "Abiogenesis" appear to present, that when Pasteur informed M. Biot that he intended to study this question, the latter assured him "you will never escape from it, you will only lose your time," whilst his teacher and friend, M. Dumas, said that he would not advise any one to occupy himself too long with such a subject. Pasteur repeated Schwann's experiments of boiling organic fluids and then allowing only air which had been passed over red-hot platinum, to come in contact with these fluids, with the result that no evidence of organic life made its appearance even in those which were most decomposable, such as urine. He also showed that on boiling the putrescible fluid in a flask with a long narrow sinuous neck left unclosed, the fluid will remain perfectly clear, and no growth will take place,

the moist sides of the narrow neck being sufficient to prevent the entrance to the flask of any solid particulate matter that may be suspended in the air. Such a flask may be kept in an incubator for months perfectly unchanged, but as soon as the narrow neck is nipped off, and particles are allowed to fall in, and with these particles the seeds or germs of moulds or bacteria, these organisms soon make their appearance, and the process of putrefaction goes on apace. M. Radot says, "lastly, to convince the most prejudiced minds and to leave no contradiction standing, Pasteur showed one of these bulbs with the sinuous neck which he had prepared and preserved for months and years. The bulb was covered with dust. 'Let us,' said he, 'take up a little of this outside dust on a bit of glass, porcelain, or platinum, and introduce it into the liquid; the following day you will find that the infusion, which up to this time remained perfectly clear, has become turbid, and that it behaves in the same manner as other infusions in contact with ordinary air.' If the bulb be so tilted as to cause a little drop of the clear infusion to reach the extremity of the bent part of the neck where the dust particles are arrested, and if this drop be then allowed to trickle back into the infusion, the result is the same—turbidity supervenes and the microscopic organisms are developed. Finally, if one of these bulbs which have stood the test of months and years without alteration be several times shaken violently, so that the external air shall rush into it, and if it be then placed once more in the stove, life will soon appear in it."

For his work on this subject Pasteur received the Prize of the Academy of Sciences, which was awarded to him "for his well-contrived experiments to throw new light upon the question of spontaneous generation".

Tyndall in this country repeated and modified many of Pasteur's experiments, and overthrew in the popular mind the theories that had been advanced by our countryman Needham, although at the time these had been successfully controverted in argument by Spallanzani. Dr. —now Sir William—Roberts, and Professor—now Sir Joseph—Lister performed a number of corroborative and

additional experiments, with the result that although Dr. Bastian made a most gallant fight in support of the theory of spontaneous generation or abiogenesis, and displayed wonderful ingenuity and ability in its defence, he had ultimately to succumb, and it is probable that in scientific circles Pasteur's demonstrations were more fully accepted as conclusive in this country than in France, and certainly than in Germany, although here Cohn attacked the theory of spontaneous generation with the greatest energy and success. It was at this point that Lister conceived the idea of his now famous antiseptic treatment. He argued that if these germs were necessary for fermentation and for putrefaction, and if they could only be developed from pre-existing germs, might it not be possible by means of mechanical and chemical appliances to prevent their gaining access to wounds in which they must necessarily be the cause of the putrefactive processes that were often developed in them? By the marvellous manner in which he developed methods of keeping organisms from the wounds, he was able to maintain an aseptic condition through which, in old erysipelas- and gangrene-infected wards, he was able to obtain results which compared most favourably with those obtained under the best hygienic conditions. His method revolutionised surgery, and has, directly and indirectly, been instrumental in saving an incalculable number of valuable human lives. Lister, like Pasteur, was able to apply practically for the alleviation of human suffering, the results of researches which in the hands of other men had remained of merely theoretical interest and value.

Having settled this question of a spontaneous generation, Pasteur next turned his attention to a more careful study of the processes of fermentation in wine, and to the wine diseases through which so much wine was rendered of little value and even unsaleable. His previous researches, of course, led him to suppose that the disease was due to some special kind of fermentation, the result of the action of an organised ferment. He found that the *Mycoderma vini* was sometimes present, setting up acid fermentation. The bitter taste sometimes developed was also traced to

organised ferments, some of which were merely moulds or mildews. He found that by carefully decanting wines from the lees, and then heating them up to about 140° F. they would keep very well, an organic ferment being destroyed at this temperature, so that if the bottles were carefully corked no decomposition would take place in the wine, which would remain in a perfectly sound condition. This process of Pasteurisation, as it is called, has been adopted with success in the sterilisation of milk, and of other substances that would be spoiled by being subjected to the action of a higher temperature.

Pasteur's researches on the silk-worm disease commencing in 1865 were the first that can be said to have definitely and conclusively proved by rigid experiment the constant connection between a particulate and living organism, and a specific disease. Pasteur's previous investigations in the domain of specific fermentations, to each of which he assigned a specific ferment-inducing micro-organism, and his further experiments from which he concluded that not only must there be a living seed from which living organisms, however low in the scale, can spring, but each form of organism must have its ultimate origin in forms like itself, had prepared his mind for this investigation. Before he commenced his work on the silk-worm farms at Alais Pasteur had never even handled a silk-worm, but at the request of his old master, M. Dumas, he determined to do what he could to help what was fast becoming a crippled industry on account of the fearful ravages of *pébrine* amongst the silk-worms. He at once turned his attention to certain small corpuscles that had already been found in diseased silk-worms and moths and their eggs by Italian Naturalists. It had even been proposed that the eggs of the silk-worms should be examined under the microscope in order that only those in which these corpuscles were absent might be hatched, but nothing had come out of this suggestion. Pasteur went to work in his usual methodical but energetic fashion. First the presence of the corpuscles was demonstrated in the egg, the worm, the chrysalis, and the moth. It might sometimes be missed in the earlier

stages of development, but in the final form, the *imago* or moth stage, it could always, if present, be easily enough demonstrated. By a series of feeding experiments the passage of these vibratory corpuscles was traced along with food into the intestine of the worm, then into the surrounding tissues; healthy worms were found to become diseased when they were invaded by these corpuscles. Should a chrysalis stage be reached, instead of a cocoon a mere mass of corpuscles might sometimes be found, whilst those that reached the moth stage were often crammed with corpuscles to such an extent that they never produced eggs at all; on the other hand, some were still capable of producing eggs, but these eggs in turn, infected from the commencement, could only give rise to diseased worms. Year after year Pasteur, assisted by a band of ardent workers, returned to the attack during the hatching season. His wife and daughter for the nonce became silk-worm farmers; forcing houses for artificial hatching were erected, and at last the "peppered" condition of the silk-worms (they are mottled by dark patches scattered over the body, hence the name *pébrine*) was traced to its true cause. It was demonstrated also that these corpuscles could find their way into worms through the wounds made in each other by means of the sharp hooks which are placed at the ends of their six fore feet, which, passing over the excreta-infected leaves, and then over the skins of their fellows, transfer the corpuscles that have been voided from the alimentary canal of an infected worm to wounds made in a healthy worm. Although drying renders these corpuscles innocuous, this must, under ordinary circumstances, be continued for several weeks, and the dry dust in the earlier stages of the process still remains capable, if blown about and carried to the mulberry leaves of unaffected farms, of spreading the disease to healthy stock.

What was to be done? The remedy was simple. As soon as the moths have paired they are set apart, each pair on a little piece of linen. As soon as the female has laid her eggs, she is pinned by the wings to the corner of the cloth where she is allowed to dry. When the leisure time of the silk-worm cultivator arrives in autumn and winter,

the dried moth is pounded in a mortar, and mixed with a few drops of water. A drop of this is examined under a microscope. If corpuscles are found, the cloth with its little mass of eggs, is, along with the contents of the mortar, thrown into the fire; if there are no corpuscles present, the eggs are kept to be hatched when the warm weather returns. In this way worms, cocoons and moths free from *pébrine* are obtained, and the disease is kept well under control.

A second disease, *flacherie*, which was found to attack weakly worms, was also traced to its source—the action of microbes taken in along with the food, and setting up a special series of fermentations in the alimentary canal.

Speaking of the precautions to be taken in the selection of silk-worms, Pasteur wrote: "If I were a cultivator of silk-worms, I would never hatch an egg produced from worms that I had not observed many times during the last days of their life, so as to make sure of their vigour at the moment when they spin their silk. If you use eggs laid by moths the worms of which have mounted the heather with agility, have shown no traces of *flacherie* between the fourth moulting and mounting time, and do not contain the least corpuscle of *pébrine*, then you will succeed in all your cultivations."

The silk industry which at one time appeared to be on the verge of extinction was restored to France, which in consequence is to-day millions richer. The prolonged strain and the work in the artificially heated cultivating rooms, appears to have told upon Pasteur's health, and in October, 1868, when still only forty-five, he broke down, being seized with an attack of paralysis. One side only was affected, but the effects remained to the day of his death. The next year, however, found him again at work at Alais, and then at the Villa Vincentina, near Trieste, on a silk farm owned by the Prince Imperial, at which for ten years the silk harvest had not brought enough to pay the cost of eggs. Under Pasteur's direction the cocoons produced "gave to the villa a net profit of 26,000,000 francs," or over £1,000,000 sterling! All opposition had speedily to give way before such a signal success, and Pasteur's method was finally adopted by the silk-worm farmers of all nationalities.

At the close of the Franco-Prussian war, during which, of course, all scientific work ceased, especially as far as Pasteur, who was an ardent patriot, was concerned, he was anxious to recommence his investigations. Again his thoughts reverted to the question of fermentation, especially in so far as it bore on an industry which he thought might be revived in his beloved country. The diseases of beer were, he pointed out, the result of the activity of lactic, butyric, and other ferments which found their way into the wort during the process of brewing, either accidentally or along with the yeast. In order to get rid of these he tried to obtain pure cultures of yeast. In this he succeeded, and so far improved the character of the ferment. Further, by applying his process of Pasteurisation—heating in bottle at a temperature sufficient to kill most non-spore-bearing organisms—he was able to prevent the development of most of the organisms accidentally admitted, with the result that he practically revolutionised the method of brewing beer, and from a mere rule of thumb process converted it into one strictly accurate and scientific. He ultimately found that when the beer is made with pure yeast and under proper precautions, no extraneous organisms find their way into it at any part of the process, and no Pasteurisation is necessary.

This study of beer, however, was merely an interlude, and Robert Boyle's prophecy that "he that understands the nature of ferments and fermentation shall probably be much better able than he that ignores them to give a fair account of divers phenomena of certain diseases (as well fevers as others), which will perhaps be never properly understood without an insight into the doctrine of fermentations," was now about to be fulfilled most completely and literally. Pasteur was not a physician or a surgeon, and it was but natural that the first practical application of his theories of fermentation to the study of disease in the human subject should be made by surgeons. In 1863 Traube in Germany pointed out that the alkaline fermentation of urine does not take place in the bladder except in those cases in which "vibrios find access to the bladder from without".

Pasteur had insisted upon this fact, and also pointed out that the fermentation was prevented by the presence of boracic acid. It was therefore proposed that in cases of operations on the bladder, the parts should be irrigated with a 3 or 4 per cent. solution of boracic acid. Mr. Guyon who had carried out this treatment at Pasteur's suggestion obtained marvellous results. In 1865, as already noted, Lister, then Professor of Surgery in the University of Glasgow, commenced his work on the antiseptic treatment of wounds. In 1874, five years after he had been called upon to occupy the Chair of Clinical Surgery in the University of Edinburgh, he wrote to Pasteur: "It gives me pleasure to think that you will read with some interest what I have written about an organism which you were the first to study in your memoir on lactic fermentation. I do not know whether you read the *British Medical Journal*; if so, you will from time to time have seen accounts of the antiseptic system which for the last nine years I have been trying to bring to perfection. Allow me to take this opportunity of sending you my most cordial thanks, for having, by your brilliant researches, demonstrated to me the truth of the germ theory of putrefaction, thus giving me the only principle which could lead to a happy end, the antiseptic system." Surgery was started on the path which has converted it from a mere handicraft to a noble art and a living science; but medicine had not yet been established in the same way although numerous observers (among them, as Tyndall points out, Dr. William Budd) indicated that the specific epidemic and epizootic diseases were due to the action of seeds or germs which cannot originate spontaneously, but which must be "bred true" from pre-existing organisms which "are only propagated now by the law of continuous succession".

This brings us to the question of "anthrax," a blood disease, extremely fatal to animals, and often attacking the human subject. It is sometimes stated that Pasteur proved the causal nexus between the *bacillus anthracis* and anthrax, but although his work undoubtedly stimulated others to investigate the subject, and to investigate it successfully,

priority of actual demonstration cannot be claimed for him. His subsequent work on anthrax was, however, perhaps even more important than that which has been claimed for him.

The history of the discovery of the anthrax bacillus and its relation to anthrax may be told in a few words. It was first seen, in 1849, in the blood of an animal that had succumbed to an attack of splenic fever. In the following year Rayer and Davaine described it as occurring under similar conditions, whilst in 1857 Brauell noted its presence in the blood of a human patient suffering from anthrax. For six years nothing more was done, but in 1863 Davaine, stimulated by the study of Pasteur's observations on fermentation, returned to his investigations on anthrax, and continued working at the subject for ten years, with the result that he was able to give everything but actual proof that the bacillus anthracis was the exciting cause of this malignant disease. At this point Koch took up the work. He applied his plate cultivation method with solidifying media, and after working for some time announced in 1876 that he had been able to furnish most rigorous proof of Davaine's hypothesis. He isolated pure cultures of the organism, described its modes of development and reproduction, pointed out that under certain conditions, like some of the organisms already described by Pasteur, it formed spores, but that it also multiplied by a process of direct division or fission. During the following year Pasteur also succeeded in growing this organism as a saprophyte. His methods are always as ingenious as they are interesting. It had been found by Davaine that the anthrax bacillus might be demonstrated in a pure condition in the blood of living animals or immediately after death, but that eighteen or twenty-four hours after death the blood was invaded by a second organism, which, inoculated, produced symptoms that could not be ascribed to the bacillus anthracis. Pasteur was able to separate these organisms by making both anaerobic and aerobic cultures from the blood in which this mixture of organisms occurred. In the presence of air the anthrax bacillus only could grow, but in the culture from which air was excluded, only the septic vibrio could develop. The one organism

requires air for its existence, on the other air acts prejudicially.

The anthrax bacillus, he found, would grow readily in certain animals, the ox, the sheep, the rabbit, the guinea-pig, not so readily in the dog, whilst he found that fowls were not at all vulnerable to its attacks. What was the reason for this difference? The answer to this question, as worked out and given by Pasteur, so well illustrates his method of reasoning and procedure that it may be given in some detail. It was found that by subjecting the anthrax bacillus to a temperature of 44° C. (111·2° F.), it was rendered incapable of developing. The temperature of the fowl—41° to 42° C. (105·8° to 107·6° F.)—though not quite so high, may help the tissues to deal with the micro-organism and prevent its development taking place in the body of this bird. "No doubt," he said, "life to a parasite in the body of an animal would not be as easy as in a cultivating liquid contained in a glass vessel." Was this temperature, *plus* the action of the tissues, the cause of the non-development of the anthrax bacillus? The now classical experiment of cooling the blood of the hen—by placing its feet in water maintained at a temperature of 25° C. until it came down to the temperature of the human body—having previously inoculated the bird with the bacillus anthracis, was made with the result that "at the end of twenty-four hours the hen was dead, and all its blood was filled with splenic bacteria". The converse experiment was then tried. "A hen was inoculated, subjected like the first to the cold water treatment, and when it became evident that the fever was at its height it was taken out of the water, wrapped carefully in cotton wool, and placed in an oven at 35° C. Little by little its strength returned; it shook itself, settled itself again, and in a few hours was fully restored to health. The microbe had disappeared. Hens killed after having been thus saved, no longer showed the slightest trace of splenic organisms."

In these experiments and the deductions made therefrom, we have the groundwork of Pasteur's great work on protective vaccines and vaccination.

Following these experiments, or rather running concurrently with some of them, came the experiments with the microbes of fowl cholera which were destined to have such a tremendous influence on the development of latter-day medicine. In fowls suffering from fowl cholera there had already been described extremely minute micro-organisms which had been supposed by Moritz, by Peroncito, and also by Toussaint to be the cause of the disease. Pasteur isolated the organism in pure culture, grew it in alkaline chicken broth, and then by introducing small quantities of the broth culture (the one-hundredth or even the one-thousandth part of a drop) under the skin reproduced the disease in the fowl and the bird died. Pasteur found that by making successive cultures at short intervals, each new one from that made twenty-four hours before, the virulence of the bacillus of fowl cholera remained unchanged. If, however, a cultivation was kept for some time (three months) protected from dust, but with free access of air, it no longer retained its full virulence, and the birds inoculated with small quantities, though they suffered some illness, did not die. If, now, after they have passed through this mild attack of the disease, they are inoculated with the non-attenuated fowl cholera virus or cultivation—which before would undoubtedly have killed them—they may have a pretty smart attack of illness, but the majority of them recover. This was the first case in which an experimenter had been able to modify a virus outside the body, and so obtain a protecting virus or one capable of inducing a mild form of disease which would protect against the attacks of a more active virus. The difficulty of obtaining control over the virulence of a disease was overcome when the modified virus had been produced. Well may M. Valery-Radot quote M. Bouley, who said when Pasteur announced his work to the Academy of Sciences: "This is but the beginning. A new doctrine opens itself in medicine, and this doctrine appears to me powerful and luminous. A great future is preparing: I wait for it with the confidence of a believer and with the zeal of an enthusiast."

It was but the beginning; ere long Pasteur was able to

produce an anthrax virus so modified by heat that it would not form spores, after which by treating the micro-organism as he had already treated the fowl cholera bacillus he was able to obtain an attenuated virus which was capable of setting up a modified attack of the disease, and so of protecting the vaccinated animal against the attack of the more virulent microbe. His successful and striking experiments at Melun have been so often quoted that it is unnecessary to go into them again, but it may be well to point out that out of this work has arisen our knowledge that vaccination against anthrax, and against certain other diseases, can protect most efficiently in most cases, and for a considerable length of time; but it is evident that there are cases in which, either through the extreme susceptibility of the patient, or the intense activity of the infecting virus, only partial protection is obtained, whilst even in the most perfectly protected individual the effect of vaccination passes off after a time and leaves the patient again comparatively vulnerable to attack.

Having reduced the virulence of the fowl cholera and anthrax organisms, the next step was to see if their virulence could be restored. In the case of the bacillus anthracis this was done by passing the attenuated form through a very young guinea-pig, which, it was found, succumbed to the disease even when inoculated with the weak vaccine; a drop of blood from this animal inoculated into one a little older causes its death, and so on until the blood taken from an aged animal which has succumbed to the inoculation anthrax suffices to kill sheep, and that from the sheep, an ox. This method of exaltation of the virulence of a virus has since then been used with great effect in the production of active toxins necessary for the production of antitoxins, now so much spoken of. Before leaving this part of our subject, it may be well to point out that it was in connection with the fowl cholera group of organisms that Pasteur was enabled to show that the products of the micro-organisms, from which the micro-organisms themselves had been removed, were capable of setting up many of the symptoms of disease, that they had in fact active toxic or poisonous properties.

This was a most important step forward, especially as Salmon and Smith, two American investigators, were afterwards able to show that in the case of hog cholera the toxins derived from the specific bacillus associated with that disease were, when injected subcutaneously in minute doses or in an attenuated condition, capable of conferring an immunity against the attacks of the micro-organism. We have in these observations the foundation of the "antitoxin" treatment of diphtheria and tetanus, although the full details of the method could not at that time be worked out or even foreseen.

Working at the question of vaccination against swine fever, Pasteur found that when the swine fever organism is passed through several generations of pigeons it gradually becomes more virulent not only for the pigeon but also for the pig. When, on the other hand, the virus is inoculated from rabbit to rabbit, these animals die undoubtedly, but the blood inoculated into a pig does not set up an acute illness in that animal; it recovers and is proof against a fresh attack of swine fever. It must be noted however that, whether this organism is cultivated in the pigeon when it is most virulent, in the rabbit when it is least so, or in the pig when it is as it were in an intermediate condition, there comes a stage in the development of the organism, when, if kept to the one species, its virulence remains stationary—it is in fact a *virus fixé*, and will so remain until transferred to another species. This most important observation was destined to bear fruit in the great work on hydrophobia to which Pasteur devoted the last years of his life. Difficulty after difficulty was overcome. Inoculation with saliva failing, through the production of mixed infections, and Galtier having failed to produce rabies by the inoculation of blood from rabid animals, inoculation with cerebro-spinal fluid and portions of the central nervous system was tried; this time the inoculations were attended with success. Then instead of making his inoculation under the skin merely, Pasteur introduced particles of the brain or cord of an affected animal under the covering of the brain of a healthy animal, with the result that the disease most

certainly manifested itself in the inoculated animal, and at an earlier period than when the inoculation was made under the skin. Applying what had been found to occur in swine fever, Pasteur found that canine virus is more powerful than virus that has been passed through a long series of monkeys, whilst that passed through rabbits gradually acquires such activity that the period of incubation of the disease,—*i.e.*, the time which the disease takes to manifest itself after inoculation—is reduced to one half. Here then it was possible to obtain materials for inoculation of all degrees of virulence. Was it not possible by a process of gradual acclimatisation of the tissues to the presence of the virus to render a patient immune to the action of the most powerful virus? This was proved, in the first instance, by the use of monkey virus, and then, virus from dogs and rabbits, but all the more recent experiments have been carried on with material derived from the spinal cord of the rabbit. Having observed that the cords of rabbits that had been dead for some time contained a less virulent poison than the cords of freshly killed animals —this being especially the case in dry weather—he came to the conclusion that he might obtain vaccines of all degrees of virulence by simply exposing the cords of rabbits that had succumbed to hydrophobia to dry air for different periods. The fresh cord would give the strongest virus, that exposed longest (fifteen days) the weakest. This was found to be the case, and dogs were protected and *cured* even when the treatment was commenced only after the animal had been bitten by a rabid animal.

The application of this method to the human subject was first made in the case of a little lad, Joseph Meister, aged nine years, who had been badly bitten by a dog. As the dog was undoubtedly mad, and as there was little chance of the survival of the child in the ordinary course of events, Pasteur resolved after consulting with Professors Vulpian and Grancher, who agreed to share the responsibility, to treat the boy as he had treated the dogs that he had already been successful in protecting. The boy recovered, and since that time there can be no doubt that, although there

may have been certain failures, a great number of lives have been saved which otherwise would inevitably have been lost.

This investigation brought Pasteur's active experimental work to a conclusion, though he was in constant attendance at the laboratories in the Rue Dutôt or in those connected with the Stables at Garches, situated in the Park of the Old Château de Ville Neuve l'Etang, where the work on hydrophobia and diphtheria is carried on, guiding, advising, directing and watching with keen interest the work of his pupils, encouraging them to overcome difficulties and rejoicing with them in their successes. He thus practically died in harness, for it was at Garches after about three weeks' severe illness—he had been ailing for some time—that he died on the 29th of September.

Pasteur married the daughter of M. Laurent, the Rector of the Strasburg Academy. His son, Jean Baptiste, is in the Diplomatic Service, and his only surviving daughter, Madame Valery-Radot, is the wife of the biographer of the great man.

It is difficult to sum up in few words the character of such a man. Those who have come in contact with him were always struck by his energy, directness and simplicity; those who have followed his work, by his industry, his pertinacity, his clear-sightedness, his wonderful powers both of experiment and of deduction, and by that power of induction which amongst scientific men is found only in those who may lay claim to genius. When, on the occasion of the celebration of his seventieth birthday, Pasteur's admirers and disciples, collected from almost all civilised nations, met to do him honour, Sir Joseph Lister, presenting what might be called an International Gold Medal, spoke to the following effect, he was but giving utterance to what every man sitting in the Sorbonne felt most sincerely. He said: "There is certainly not in the whole world a single person to whom medical science is more indebted than to you. Your researches on fermentation have thrown a flood of light which has illuminated the gloomy shades of surgery, and changed the treatment of wounds from a matter of doubtful, and too often disastrous, empiricism into a scientific art, certain and

beneficent. Owing to you surgery has undergone a complete revolution. It has been stripped of its terrors, and its efficiency has been almost unlimitedly enlarged. But medicine owes as much to your profound and philosophic studies as surgery. You have raised the veil which had for centuries covered infectious diseases. You have discovered and proved their microbic nature; and thanks to your initiation, and in many cases to your own special labour, there are already a host of these destructive diseases of which we now completely know the causes. This knowledge has already perfected in a surprising fashion the diagnosis of certain plagues of the human race, and has marked out the course which must be followed in their prophylactic and curative treatment. Medicine and surgery are eager on this great occasion to offer you the profound homage of their admiration and their gratitude."

Whatever may be Pasteur's claim to honour, and how fully that claim is accepted Lister's words convey, it will in future perhaps, even more definitely than it does at present, undoubtedly rest upon the work which he has done in elucidating the questions connected with the ætiology and prophylaxis of specific infective disease. This one idea is the root from which his work branches out in all directions. One line of thought, work, and reasoning was followed throughout. Each step in this path was carefully probed and tested before any further advance was made, and once gained, was never left until it had been made thoroughly sound as a point from which the next step might be made. This it is that makes his work so reliable, and assures for it the immortality to which only truth can attain.

Pasteur was a patriot, and France is to-day poorer than when her great son was alive; but the loss that science of all nations has to bear is infinitely greater, not because his work will perish, for that can never come to pass, but because he can no longer work except through those who during his lifetime have been his pupils, friends, and assistants. They, however, have already given proof that we may not look in vain to them for the continuation of the noble work that their master commenced.

GERMAN SIMS WOODHEAD.